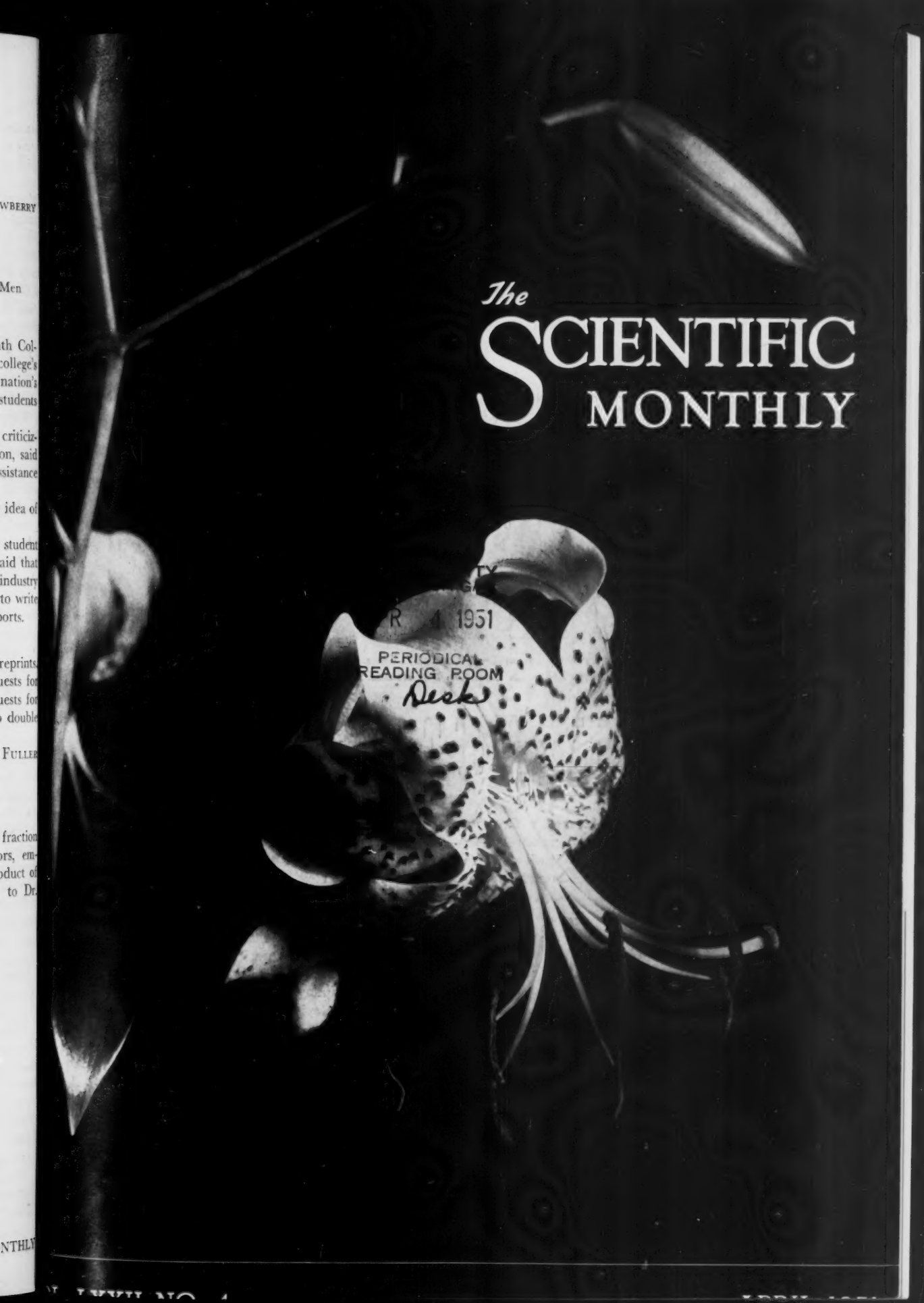
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VOL. LXXII APRIL 1951 NO. 4

Contents

Lilium Speciosum, "Queen of the Lilies" . . . . . Cover
(Photo by Lillian Guernsey, Bureau of Plant Industry, Soils, and Agricultural Engineering, Beltsville, Maryland)

Recent Developments in Lily Breeding Techniques . . . . . *S. L. Emsweller* 207

Cone Nebula (*Verse*) . . . . . *Hal Tucker* 216

Evolution of the Quantum Theory . . . . . *E. U. Condon* 217

What Is a Spinor? . . . . . *F. D. Murnaghan* 223

The Application of Ecological Knowledge to Forestry . . . . . *Henry I. Baldwin* 225

Wildlife Management and Ecology . . . . . *Gustav A. Swanson* 230

The International Map of the World . . . . . *Dean S. Rugg* 233

The 1949 Eruption of Ngauruhoe . . . . . *Preston E. Cloud, Jr.* 241

Karl Marx and the Social Scientists . . . . . *Margaret T. Hodgen* 252

Science on the March:
Industrial Limestone in Indiana . . . . . *John B. Patton* 259

Book Reviews by *H. H. Bartlett, Philip B. King, Herbert B. Nichols, A. C. Walker,* and *J. S. Wade* 266

Letters from *Garland Ethel, E. L. Gayhart, Vivien Morgan Hone, Eugene H. Payne, Donald Culross Peattie, William A. Shurcliff, John F. Steinman,* and *Ralph Hazlett Upson* 271

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Science and Technology

(From the Month's News Releases)

Away From It All

For nearly 20 years, groups of "Trail Riders of the Wildnerness" have gone into little-known areas of our Western national forests and parks, to discover at first-hand the charm and majesty of the last American frontiers. This year the American Forestry Association is organizing 13 tours, the first to begin June 4 and the last to end September 10. Costs range from $107 to $205 for about ten days of splendid solitudes. Anyone in good health and with a reasonable amount of riding experience, capable of adapting himself to living under wilderness conditions and at high altitudes, may join the Trail Riders. For further information and application forms, write to the Association at 919 17th St., N. W., Washington 6, D. C. For those who wish to do their own planning, the Forest Service offers a small booklet, *National-Forest Vacation,* which supplements road maps by listing main access highways, and the location of the headquarters and many of the ranger stations of the national forests.

To Take with You

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For Brighter Eyes

An eye-pad mask—for hot or cold application—is made of soft, durable Vinylite plastic with a sealed-in chemical solution formulated to retain heat or cold for 30–45 minutes. The plastic is resistant to oils, chemicals, and moisture and may be cleaned with sponge or damp cloth.

Automatic Deodorizer

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Research Is Big Business

Chicago Natural History Museum will send sixteen expeditions into the field in 1951. Largest will be the Southwest Archaeological Expedition, which will continue its work in New Mexico. Most work will be done in the United States, but members of the staff will wander as far afield as San Salvador and French Equatorial Africa.

Soil Pasteurizer

A portable drier and heater for road-patching materials has been adapted to the pasteurization of greenhouse soils. It may be used outodoors, or in a shed if certain precautions are observed. The average greenhouse operator can pasteurize enough soil for about 250 flats in about three hours, at a cost of less than half a cent per flat for fuel.

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An outdoor blanket that keeps the body warm and is weather-resistant consists of a pliable plastic which resists moisture, salt water, and mildew, and is nonflammable. The underside is made of soft dynel blanket material, warm as wool; the blanket thus provides unusual warmth combined with lightness in weight. Ideal for marine or camp life, it is also useful as a lap robe or as a crib or pram blanket. Available in maroon, navy blue, or gray.

Publications Noted

Modern Latin America in Social Science Literature, Part II. An annotated bibliography of books, pamphlets, and periodicals in English in the field of economics, politics, and sociology of Latin America. In Spanish and English.

Composition of Foods (147 pp.) lists 751 food items, from dried almonds to brewer's yeast. Foods are listed in three tables: one showing nutritive values of each item in terms of a 100-g edible portion; one showing values in terms of 1 pound of the food as procured for eating; and one showing nutritive values of foods in terms of common household units, such as one cup. Single copies free to nutritionists and others who calculate diets. 35¢ per copy to others, from the Supt. of Documents, Washington 25, D. C.

The German Chemical Industry, a *Bibliography of the Chemical, Metallurgical and Process Industries,* produced by the Army Chemical Corps. $10.00 from Office of Technical Services, U. S. Department of Commerce.

A Color Atlas of Pathology, result of six years of work by the Naval Medical School, has been published by Lippincott, of Philadelphia.

The *Journal of the Franklin Institute* celebrated its 125th year of continuous publication with a special anniversary issue in January of more than 200 pages. Big feature is a symposium of fifteen papers entitled "Science and Tomorrow," by authorities in the basic physical sciences, applied science, and technology. W. F. G. Swann, of the Institute's Bartol Research Foundation, concludes the symposium with a paper on cosmic rays. Copies of the issue may be obtained for $1.00 from the Franklin Institute, Philadelphia 3.

The 1951 edition of *New Products and Services* is a compilation of more than 1,000 innovations just marketed by more than 750 manufacturers. Each product is fully described and cross-indexed. The 80-page booklet is published by the New York *Journal of Commerce,* 63 Park Row, New York City 15.

Stainless Steel Stirrer

A new umbrella stirrer has four blades that can be closed to pass through the neck of a narrow-necked flask, then opened for stirring. The stirrer speeds up dissolving action, preparation of slurries, and mixing of emulsions, and seems particularly desirable for use with a three-neck flask.

Two-Pen Recorder

An electronic recorder registers two functions simul-

[*Continued on page vi*]

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[*Continued from p. iv*]

taneously against time, one in red and one in black, thus saving tedious compilation and point-by-point plotting of data. Overlapping or side by side, pens operate either across full scale or on a specified portion of the full width. Speed of response for full-scale pen movement is one, two, or three seconds, as specified.

Tipplers Undone

Bad news for drunken drivers, whose condition could formerly seldom be established in court, is a portable, automatic laboratory called an Alcometer. The subject exhales into a tube, and in a few minutes whatever alcohol is in the breath reacts with iodine pentoxide, producing free iodine. The intensity of the color produced by the iodine in a fixed volume of starch and potassium iodide solution determines the amount of alcohol in the blood stream, which is recorded in percentages. The procedure, developed after 11 years of research at Yale's Center for Alcohol Studies, eliminates all human error.

Better Gardens

Plastic film bags, coverings, and linings to protect seedlings, keep roots untangled, facilitate controlled plant feeding, simplify changing of crops, and keep work areas sanitary and maintenance costs low promise increased yields from greenhouse gardening. Bags are filled with mica and set on a board in a long, narrow wooden trench lined with the plastic film. Through openings in the bottoms of the bags, the plants absorb chemical nourishment and water automatically pumped into the trenches at proper intervals. The yield of tomatoes, for example, has been considerably increased by this process.

Foreign Study Opportunities

Study Abroad, an international handbook of fellowships, scholarships, and study grants, lists more than 30,600 foreign study opportunities. Volume III of this annual UNESCO study guide lists awards available from donors in 54 countries, from the dependencies and trust territories of five governments, and through the United Nations and its specialized agencies. Copies are available from Columbia University Press, 2960 Broadway, New York, for $1.25.

Better than Chamois?

X-lint, a long-wearing, nonwoven fabric, impregnated with Hycar latex, which looks, feels, and absorbs water like chamois, but wears three times longer, is available in an 18″ × 19″ size for approximately one third the cost of natural chamois. According to the manufacturer, the cloth is unaffected by ordinary household chemicals and solvents, leaves no lint, and is impervious to salt water.

Safer Oil Burners

An electronic combustion safeguard system for commercial oil burners, which provides immediate cut-off of fuel in case of failure, includes a switch-amplifier-contactor unit, phototube holder, and electrode holder. The system assures positive protection against combustion failure because if the gas pilot does not ignite, the oil valve cannot open, and if the oil flame does not ignite, the supply of fuel is cut off. The motor continues to operate for 30 seconds after cutoff, and the system is then locked until manually reset. For large motors and multiphase operation, magnetic switches are available.

Aid for Surgeons

Surgical drapes made of a tough plastic film that is resistant to moisture and alcohol, surgical solvents, and detergents come already sterilized, ready for instant use. They adhere to the skin on contact anywhere on the body, eliminating the need for skin clamps and providing a large, flat, sterile field. Packaged individually, the drapes can be stored indefinitely, are disposable after use, and come in four sizes and types.

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THE
SCIENTIFIC MONTHLY

APRIL 1951

Recent Developments in Lily Breeding Techniques

S. L. EMSWELLER

The article by Dr. Emsweller has been adapted from his presidential address before the American Society for Horticultural Science in Columbus, Ohio, last September. The author has been in charge of the section on Floriculture and Ornamental Horticultural Investigations at the Department of Agriculture (Beltsville, Md.) since 1935. He took his Ph.D. at the University of California, specializing in genetics and cytology. In addition to being twice the recipient of the Vaughan prize, he has recently been awarded the Gold Medal Award of the New England Gladiolus Society for his research on gladiolus.

THE lily research program in the United States Department of Agriculture is concerned with all phases of lily production—including propagation, forcing, nutrition, control of diseases, and breeding new varieties. Valuable contributions to this program have been made by various cooperating agencies: Louisiana State University, Oregon State College, West Virginia University, and many members of the Society of American Florists and Ornamental Horticulturists.

Prior to World War II, practically all the lily bulbs forced or grown in gardens in this country were imported, about 27 million being brought in in 1940. The curtailment of these importations stimulated the development of domestic sources for these bulbs, and at present most of the lily bulbs used in this country are being produced here. About 85 per cent of all our lilies are Easter lilies, and their present value on the production level is about $6,000,000. When forced by florists they are valued at about $12,000,000 and have a retail value of about $32,500,000.

Easter lilies are in demand every day in the year, and in order to meet this demand it was necessary for the Department of Agriculture to work out preplanting cold storage schedules. As a result of Stuart's[1-8] work it is now possible for florists to flower Easter lilies at any specified time. Stuart also demonstrated that lilies could be stored at temperatures just below freezing and when so handled would maintain a high flower production after storage for as long as a year. Previously it was considered unsafe to store Easter lilies much below 35° F.

Although the propagation of lilies by scaling is a process long known to plant propagators, it has very seldom been used for large quantities, because of serious losses from rotting. In our research pro-

Easter lilies of the Estate variety grown at Brookings, Ore. World War II abruptly terminated imports of Easter lilies from Japan, and within a few years American growers had developed a domestic source of lily bulbs of which the above show a good representative field.

gram, McClellan and Stuart[9] have developed control measures that have practically eliminated rotting as a limiting factor in bulb scale propagation.

The most serious problem facing the producer of Easter lily bulbs is the rapid spread of virus diseases. This was one of the first problems encountered in our lily program. In a series of papers, Brierley and Smith[10,11] demonstrated that the necrotic-fleck virus of Easter lilies and the mottle viruses were distinct entities. They further showed that fleck was the symptom expression of the joint interaction of a lily-symptomless virus and a cucumber-mosaic virus. As a result of their work, lily growers now know what to rogue for and what vectors should be kept under control.

In this article emphasis is placed on our lily-breeding program, but it will include also a discussion of research closely allied with it, upon which most of our breeding work with lilies is dependent. The genus *Lilium* comprises approximately 80–85 species, found, with few exceptions, in the North Temperate Zone of America, Europe, and Asia. The few species not indigenous to this area are located north of the equator in the tropics of India and the Philippines. At present only a few lily species are of commercial importance: notably *Lilium longiflorum* Thunb., the Easter lily; *L. regale* E. H. Wils., the regal lily; *L. auratum* Lind., the gold-band lily; *L. candidum* L., the Madonna lily; and *L. speciosum* Thunb., the beautiful pink-spotted lily. There is, however, a growing interest in lilies as garden subjects, and other species and new hybrid varieties are now increasing in importance.

Many problems confront the lily breeder, and advancement in this field has been retarded pending their solution. Among these are self-incompatibilities within clons, and cross-incompatibilities between species. Self-incompatibilities have received most attention in the species *L. longiflorum*,

the Easter lily. This species is composed of several clons comprising the domestic varieties Creole, Croft, Georgia, Ace, and Estate and the Bermuda variety, *Harrisii*. Two Japanese varieties are known in the trade as giganteum and Erabu, but neither is a clon, since there are several morphologically distinct types of each.

Several investigators have reported successful intercrossing of *L. longiflorum* varieties, and many failures with self-pollinations. Oliver[12,13] in 1903 and in 1908, reported success with the cross *Harrisii* × *giganteum* and *L. longiflorum* (variety not specified) × *Harrisii* and the reciprocal. He also reported that a low seed set was obtained from *Harrisii* selfed. Griffiths[14] reported that *L. longiflorum* is usually self-sterile unless there is a wide variation in the parental varieties. Stout[15] self-pollinated 27 species, including *L. longiflorum* and some hybrid varieties, and found self-incompatibility prevalent in all instances.

Brierley *et al.*[16] found that the Creole, Croft, and Estate varieties of Easter lilies all failed to set seed when self-pollinated. Later experiments showed that the Georgia variety was also self-incompatible, but the Ace variety was slightly self-fertile.

The fact that self-incompatibility is so common in the genus *Lilium* makes it mandatory that most lily varieties and species be propagated asexually, since it is impossible to develop pure breeding lines by inbreeding. If the establishment of such lines were possible, they would be especially helpful in maintaining virus-free varieties, since these diseases are not seed-borne in *Lilium*. Such lines would also be valuable for genetic studies, and their absence in *Lilium* probably explains the almost complete lack of genetic data in this interesting group of plants.

Recently Emsweller and Stuart[17] have overcome some of these self-incompatibilities in *L. longiflorum* by using growth regulators. A 1 per cent solution of naphthalene acetamide in lanolin was applied to a wound induced at the base of the pistil by breaking a petal connection at that point. The Creole, Croft, and Ace varieties were included in the experiment. Fruits and seeds were obtained on the Creole and Croft varieties only when the growth regulator was used. In the Ace variety 48 untreated pollinations produced 9 fruits, which contained 207 seeds with embryos; 90 treated pollinations produced 90 fruits, with a total of 2,414 good seeds. In later experiments self-pollinations of the Georgia lily yielded seed only when a growth regulator was used.

In a series of self-pollinations in a population of lilies derived from crossing *L. tigrinum* × a mixture of pollen from 7 other species, a beneficial effect of the growth regulator in overcoming self-incompatibilities was obtained. In 1948, 22 plants of this population flowered outdoors, and 72 capsules, containing 2,539 seeds with embryos, formed from open pollination. In 1949, on this same population, 209 controlled pollinations, including selfings, were made without use of growth regulator, and only one seed was obtained. In 1950, 21 of the same 22 plants were grown in the greenhouse, and each flower was self-pollinated; about half the flowers on each plant were treated with the growth regulator, and others were not treated. One plant produced 4 seeds from untreated, and 41 seeds from treated, flowers. None of the remaining 20 plants produced seed from the untreated pollinations, but 8 yielded 265 seeds from the treated pollinations.

Cross-fertilization between some species of *Lilium* have also been obtained only when a growth regulator has been used. In all species of *Lilium* tested, the treatment induces formation of fruits, but in some instances where cross and self-fertilizations

These lilies were grown from bulbs dug at the same time, but stored for different periods of time at 50° F. Storage intervals from left to right: no storage, two, four, six, eight, and ten weeks at 50° F.

are normally successful, the use of a growth regulator actually reduced the amount of seed. Results to date also indicate that some types of sterility in *Lilium* are not affected by the growth regulator. No explanation can be offered at this time as to how a growth regulator effects such results. Possibly it speeds up growth of the pollen tubes, or delays senescence of the embryo sacs.

Another difficulty that has handicapped lily breeders is the phenomenon of alleged apomixis in many species of *Lilium*. Lily breeding literature[18-20] contains many references to apomixis, especially in the case of *L. regale* E. H. Wils. This species has been reported to set seed readily when pollinated with almost any other species of *Lilium*. The resulting seedlings, however, were reported to closely resemble the maternal parent, *L. regale*. It is now so commonly believed that this species sets apomictic seed when cross-pollinated with other species that lily breeders refrain from using it as a maternal parent. Recently Rappleye[21] has carried out a series of carefully controlled cross-pollinations on *L. regale*, using 21 different species as pollen parents. Included among his pollen parents were many of the species reported by other investigators as effective in inducing apomixis in *L. regale*. Rappleye, however, obtained no seed from any of the cross-pollinations, with the exception of *L. leucanthum* var. *chloraster* (Baker) E. H. Wils.

Large seed pod at top right was obtained when a growth regulator was applied at the base of the flower at time of pollination. Small undeveloped seed pod at left was also pollinated, but a growth regulator was not used.

In a series of pollinations on *L. regale* hybrids made at Beltsville, we obtained results similar to those of Rappleye. Our *regale* hybrids were derived from interpollinations between *L. regale*, *L. leucanthum*, and *L. Sargentiae* E. H. Wils. These three species are very closely related, and hybrids between them have been obtained by many breeders.

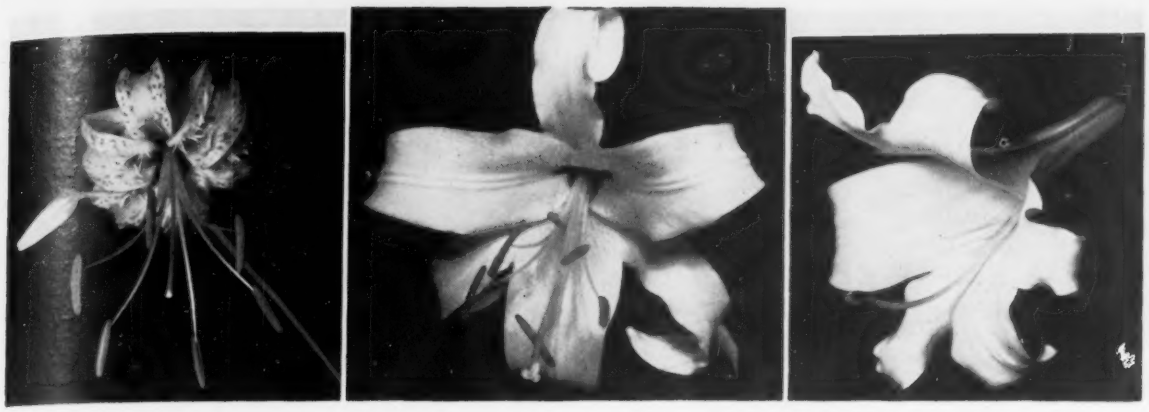
Other investigators have reported obtaining hybrid seed on *L. longiflorum* when pollen of certain other species has been used. The seedlings produced here all closely resembled *L. longiflorum*. At Beltsville we have not obtained any seed on this species when 10 different species were used as pollen parents over a thirteen-year period.

In some reported instances of apomixis in *Lilium* it is now known that the maternal parent was not isolated from pollen of other lilies of the same species. In such cases the assumed apomictic seed set is very probably the result of crossing with sister plants in the same population. Such evidence indicates that apomixis is less prevalent in *Lilium* than heretofore suspected, but there are so many reports of this condition in the literature that further work is necessary to clarify the matter.

For some time now we have been using soda straws to isolate emasculated lilies from contamination by undesired pollens. The straws are cut into convenient lengths, one end compressed and folded over, and the open end slipped over the stigma and style. The petals are then folded over the straw and held in place with a rubber band.

When it is desired to cross early- and later-flowering lily species it is necessary to store pollen until the maternal species is in flower. Pfeiffer[22] demonstrated that pollen of *L. auratum*, *L. longiflorum*, *L. philippinense* Baker, and *L. speciosum*, stored in a desiccator at 10° C in humidities of 35, 50, and 65 per cent, remained viable for about nine months. We are now drying lily pollen thoroughly, placing it in a glass vial with cotton plug, and storing the vial in a desiccator at a temperature of 48°–52° F. Lily pollen stored in this way has remained viable for as long as four months. Its viability is easily checked by germination in a drop of lily stigmatic fluid placed on a glass slide.

There is so much variability in foliage characters within a group of seedlings of many *Lilium* species, and the seedlings of different *Lilium* species



Left, *Lilium henryi*; center, the hybrid obtained by crossing *L. myriophyllum* × *L. henryi*; right, *L. myriophyllum*.

so closely resemble one another, that it is not possible to determine hybridity by superficial inspection before flowering. Since this requires from two to five years from the seed, a more rapid method is essential to save time and labor. The large chromosomes of *Lilium* have long been favorite subjects for demonstration purposes, but it is only recently that improved cytological techniques have made it possible to determine their morphology accurately. Utilizing chromosome morphology, Emsweller and Stewart[23] were able to determine the parentage of × *L. testaceum* Lindl. This lily is reported to have been found in 1836 in a planting of mixed *L. martagon* L. growing in a German nursery.[23] For some time it was considered to be a new species from Japan, but lily fanciers shortly considered it to be a hybrid, probably between *L. candidum* L. and *L. chalcedonicum* L. The chromosome number of × *L. testaceum* was found to be 24, as is typical for all lily species so far counted, with the exception of the triploid *L. tigrinum* Ker-Gawl and a few species in which centric fragments are also present. Since the putative parent species of × *L. testaceum* are *L. chalcedonicum* and *L. candidum*, several types of these two species were obtained and the morphology of their chromosomes was determined. Each species was found to contain several chromosomes that differed distinctly from any in the other. It was relatively easy to determine that the × *L. testaceum* chromosomes consisted of 12 from *L. chalcedonicum* and 12 from *L. candidum*, thus proving beyond doubt that these two species were the parents of × *L. testaceum*.

A somewhat similar problem was also solved by Emsweller and Burrell,[24] who, by means of differences in chromosome morphology, determined the paternal parent of a lily hybrid of rather recent origin. This hybrid, known as × *L.* T. A. Havemeyer, was introduced by the originator as arising from a cross-pollination between *L. myriophyllum* Franch. × *L. tigrinum* Ker-Gawl. The alleged pollen parent was soon questioned by some lily fanciers, who postulated *L. henryi* Baker as the actual paternal parent. Pfeiffer [25] made this latter cross and obtained hybrids so similar to × *L.* T. A. Havemeyer that *L. henryi* was generally accepted as the pollen parent. An examination of the chromosomes of × *L.* T. A. Havemeyer, *L. myriophyllum*, and *L. henryi* proved beyond doubt that *L. henryi* is the pollen parent of this hybrid.

The chromosomes of 39 species of *Lilium* have now been studied (Stewart,[26] Emsweller and Stewart[23]), and with few exceptions it would be possible to determine hybrids between these species by means of differences in chromosome morphology. If the maternal parent is known, the problem is much simpler, and in practically all instances of suspected apomixis it is now possible to determine rather quickly from smears of seedling root tips whether true hybrids have been obtained.[27]

Lily breeders have encountered considerable difficulty in germinating seed of many lily species. It is now apparent that lily species may be separated into two types with regard to the requirements for the germination of their seed. In one group, including such species as *L. longiflorum*, *L. regale*, and *L. pumilum* D. C., seeds germinate quickly and seedlings emerge within two or three weeks. In the other group we have such species as *L. auratum*, *L. canadense* L., and *L. speciosum*, which usually do not appear above ground for eighteen to twenty months after fall planting. Barton[28] has shown that this delay in emergence may be greatly reduced by exposing the seed to a temperature of around 65° F. for about three to six months, followed by exposure to temperatures of 35°–50° F. for six weeks to three months. During the first part of this treatment the root starts to develop and may reach a

Chromosomes of a lily plant, showing all 24 at mid-metaphase in a dividing root tip cell. There are 12 types present.

length of a few millimeters, and in some species a tiny bulb is also formed. This growth takes place at the expense of the reserves stored in the endosperm. During this period the small bulb is practically inactive and remains so unless exposed to lower temperatures. At the lower temperatures, the dormancy is broken and shoot growth is initiated when the bulb is returned to higher temperatures.

For several years we have been starting the slow-germinating types of lily seed in small glass jars that can be stored in controlled-temperature rooms. The seed is sterilized by soaking overnight in a saturated solution of Tersan, then distributed in slightly moistened peat or a fine grade of vermiculite, and this mixture is put into a glass jar. If peat is used, it is mixed with Fermate at the rate of about one pint of Fermate per bushel of peat. Vermiculite is already sterile and is used without treatment. The mouth of each jar is then covered with a sheet of polythene, held in place by a rubber band, and the jar stored at 65°–67° F. The jars are examined in a month or so, and any jars containing seeds that have produced roots or tiny bulbs are transferred to a temperature of 32°–35° F. After about two or three months of exposure to this temperature, the germinating seeds or tiny bulbs are removed and planted in flats, with vermiculite as the growing medium; they show top growth in one or two weeks. The time required for emergence has thus been reduced from eighteen to twenty months to four to six months.

In a series of germination tests at Beltsville with *L. auratum,* Pryor[29] found that about 65 per cent of the seed from green undehisced pods produced seedlings in six weeks when planted in soil at temperatures of 65°–70° F., whereas no seedlings emerged from seed of fully matured and dehisced pods for about nine months, and then only following storage for three to four months at 65°–70° F., followed by an equal period at 32°–35° F. This indicates that dormancy may be induced by some substance either synthesized in the seed as it matures or translocated to the embryo during its maturation. There is no direct evidence to support either of these possibilities, and some other process may be involved.

During the past few years we have found that some of the slow-germinating lily species require relatively short periods of low temperatures to break dormancy, whereas others are very erratic in their behavior. Some of the latter types of seeds may start growth, forming tiny bulbs or roots within six weeks following the cold storage treatment; others start a month later; and some require as long as twelve or more months or a second cold treatment. There is considerable disagreement in the literature regarding the classification of certain lily species as to whether they germinate quickly or slowly, and this is now understandable from some of the results that we have obtained.

Prior to the discovery by Stuart[30] that vermiculite was such an excellent medium for growing seedlings, we had considerable difficulty in obtaining a high emergence and survival rate of lily seedlings in soil. All our lily seeds are now examined before planting by placing them on a glass surface illuminated from below, and those lacking embryos are discarded. In some instances we now plant about 500 seeds per flat in vermiculite and obtain 475–495 seedlings. Those species that grow slowly are left in the flat for two years. When the foliage matures it is cut, vermiculite is added to fill the flat, and the flat is then stored at 31°–32° F. for three to four months. When returned to the greenhouse, the seedlings resume growth in a few weeks, and large planting-size bulbs are obtained in eight to twelve months. This is much sooner than such bulbs can be produced outdoors. The vermiculite flats are all subirrigated, and nutrients are applied in the water. About every six weeks a saturated solution of Tersan is applied; this has practically eliminated bulb rotting.

When colchicine became available we immedi-

ately began using it to induce polyploidy in *Lilium*. The first treatments were applied by soaking the lily seeds in various concentrations of colchicine, but no affected seedlings were obtained. Later,[31] the tip of a flowering stem of *L. formosanum* Wallace was immersed in colchicine, and from this treatment aerial bulblets were produced, a fairly high percentage of which were tetraploids.

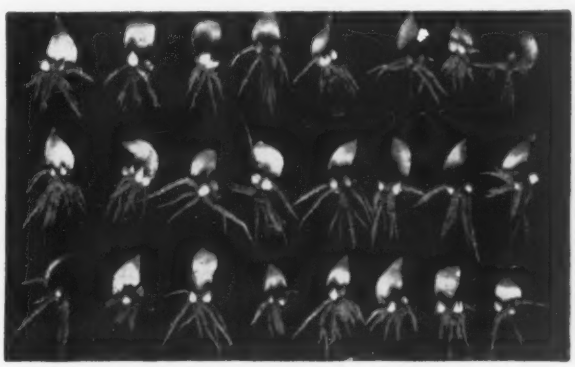
The next treatments with colchicine on *Lilium* were applied to bulb scales of *L. longiflorum* and of certain other species. Propagation of lilies by means of scales is an easy and rapid method of increasing a new lily. Each detached scale produces from one to many adventitious bulblets within four to six weeks. When freshly detached scales are immersed in an aqueous colchicine solution, a fairly high percentage of the bulblets formed is polyploid. These polyploid lilies are composed of balanced tetraploids, with all cells containing 48 chromosomes; various types of aneuploids, such as 4n-1, 4n-1-1, 4n-1-1-1, and 4n + 1; and chimeras of many types.

Several clons of *L. longiflorum* have been made tetraploid, and some are proving to be better than their diploid form. As a rule the tetraploid forms of *L. longiflorum* are more vigorous in all vegetative characters and bear larger flowers of greater substance. In some clons, fewer flowers are produced from tetraploid bulbs than from diploid bulbs of the same size; in other clons the average number of flowers per bulb is the same.

It is impossible to characterize a balanced tetraploid lily plant, since chimeral plants usually appear identical with them and cannot be differentiated. Periclinal chimeras, with a diploid epidermis and a tetraploid stele, can be distinguished from balanced tetraploids by their smaller stomata; and plants with a diploid stele and tetraploid epidermis bear small diploidlike flowers. Chromosome counts of root tips of lilies derived from bulblets produced on scales treated with colchicine are of no value in characterizing the ploidy of the stem, since roots and bulblets, as shown by Walker,[32] are produced independently from separate groups of cells. Thus a lily may have a diploid root and a tetraploid top, or the reverse.

Some of the chimeras resulting from lily scale treatments with colchicine are more difficult to detect and analyze than others. Emsweller and Stewart[33] have recently shown that chimeral plants are produced in which both tetraploid and diploid pollen mother cells occur in the same anther, and that such plants may exhibit this condition for at least two successive years. They selected 12 *L. longiflorum* bulbs of one clon, from a large group derived from colchicine-treated scales. Each bulb had a 4n root system, and there were also several stem bulblets with each mother bulb. Scales were removed from each and propagated, and the 12 resulting populations grown in the greenhouse for observation. The mother bulbs, the stem bulblets, and a few of the bulbs from scales flowered, and meiosis was observed in some flowers of all 12 sister populations. All the flowers produced on eight of these lines had 24 chromosomes and normal diploid meiosis. Three populations had 48 chromosomes in meiosis, and one population had some plants with diploid meiosis and other plants in which both tetraploid and diploid pollen mother cells occurred in the same anther. When this condition was observed in a flower, all remaining anthers were examined, and it was found that the ratio of tetraploid to diploid pollen mother cells varied from anther to anther. A plant-by-plant examination of additional clonal lines presumed to be tetraploid disclosed this same type of chimera in four others. One plant was found in Clon 10 in which one anther of a flower contained tetraploid pollen mother cells only, and the four other anthers available contained 26, 30, 45, and 73 per cent tetraploid pollen mother cells, the remainder diploid.

Chimeral lily plants such as those just described have the gross morphology of tetraploids, and they are detectable only by cytological examination and by their fertility in compatible crosses with diploids yielding diploid progeny only, whereas tetraploids of the same clon are sterile. Without cytological evidence such plants could be reported as reverting to diploids, whereas their fertility depends on the diploid gametes produced by them. As yet we do not have sufficient data to classify these plants, but they are probably periclinal chimeras in which cells from the epidermis have occasionally, by tangential division, formed sectors in the hypodermal layer during the early ontogeny of an anther.



Lily scales with bulblets and roots, eight weeks after scales were removed from the mother bulb.

Colchicine treatments of *Lilium* are also inducing morphological changes that cannot be attributed either to tetraploidy or to the types of chimeras so far analyzed. In one such instance a plant of *L. longiflorum* derived from colchicine treatment exhibited all the outward characteristics of a tetraploid, but had diploid roots and diploid meiosis. After three successive periods of growth and flowering it is practically impossible other than by cytological techniques to distinguish this plant and its asexual progeny from tetraploids of the same clon, except that it is fertile and its seedlings are diploid. This same condition also occurred in the *L. formosanum* plants derived from colchicine treatments. Some of the large-flowered tetraploidlike plants were later found to be diploids in both roots and stem. In another instance scales of a species hybrid × *L.* T. A. Havemeyer were treated with colchicine; several plants derived from the treatment were very vigorous and bore large flowers of heavy substance. In 1949 one of these flowers set 26 seeds when pollinated with a mixture of its own pollen and that of *L. myriophyllum*, one of its parents. This was the first time we ever obtained seed on *L.* T. A. Havemeyer, but we were successful again last year with pollen of *myriophyllum*. The × *L.* T. A. Havemeyer plants have all had diploid roots, and meiosis has also been diploid. Some seedlings from the 1948 pollinations have now been examined; all are diploid. It remains to be seen whether these large diploid plants of *longiflorum* and Havemeyer maintain their vigor in succeeding asexual generations. It is possible they may be a type of dauermodification that will disappear in later asexual generations.

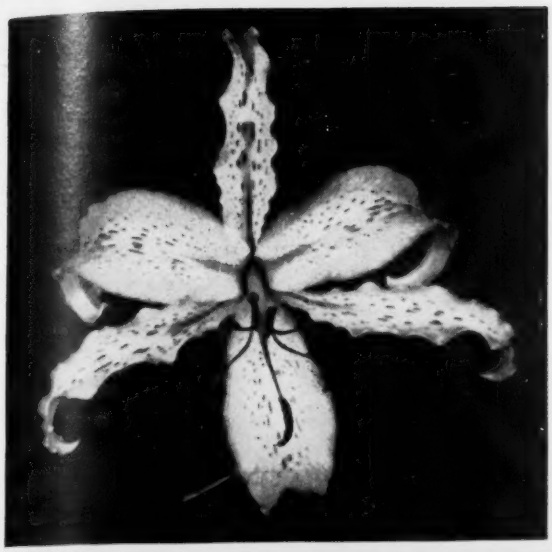
In another instance following colchicine treatment of *L. longiflorum* scales of Clon 29, several plants were obtained that do not fit into any of the polyploid or chimeral types known to us. The morphology is so modified that some plants resemble other species more closely than they resemble *L. longiflorum*. They have maintained this condition for several asexual generations and are being grown on for further observation.

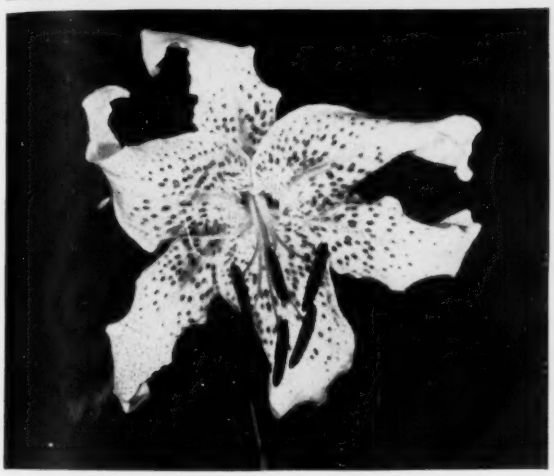
At present our lily breeding work is concerned with developing improved types of *L. longiflorum* for Easter and for cut lilies all through the year. A tetraploid Easter lily that flowers on a 14- to 20-inch stem is now being increased and will probably be named and released this spring. Its introduction had to await the necessary cytological work to be certain it was all tetraploid and not a mixture of chimeral types. It was obtained by colchicine treatment of a seedling that had very desirable characteristics except that the flowers were small and lacked substance. The tetraploid has retained all the desirable characteristics and added flower size and substance. Several tall Easter lilies for cut flowers are also being increased and should appear in commerce very soon.

In garden lilies we are making numerous species crosses and also developing tetraploids and amphidiploids. One of the most beautiful of all the hybrids with which we are working is the one between *L. auratum* and *L. speciosum*. A hybrid between these two species was first obtained in 1869 by Parkman,[19] the well-known American historian. His hybrid created a sensation when it was exhibited in England. Unfortunately, all the stock was lost because of bulb rot—probably *Fusarium oxysporum* f. *lilii* Imle. Many investigators have tried to repeat this cross, and in recent years several hybrids between *L. auratum* and *L. speciosum* have been reported. Since the two species are heterozygous and exist in many varieties, the hybrids are variable, but are exceedingly worth-while lilies. We are now obtaining some of our most beautiful and promising garden- and florist-type lilies by backcrossing these hybrids to their parents. When crossed back to *L. auratum*, the resulting progenies consist of plants bearing flowers similar in form to *L. auratum* but differing in color from any now known in this parent species. In backcrosses to *L. speciosum*, flowers of distinctly new forms and colors are being obtained.

At the present time most of the lilies found in gardens are unmodified species, and very few named varieties are available. This condition is changing very rapidly, however, and it is probable that within a few more decades named varieties of

Diploid Easter lily flower at left, and tetraploid of same lily at right. All cells of the diploid lily contain 24 chromosomes, all those of the tetraploid, 48 chromosomes.



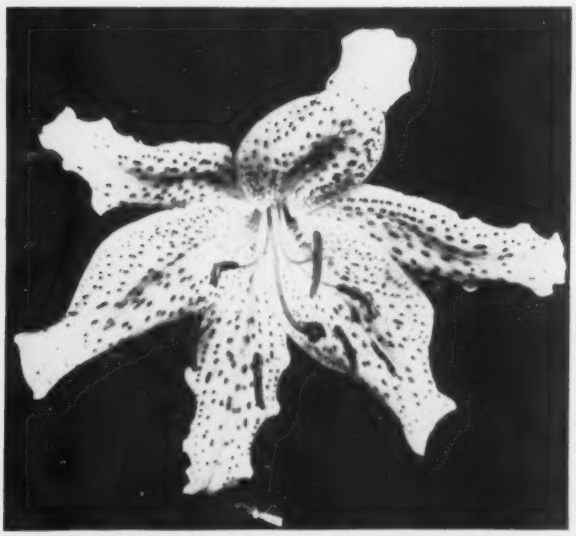


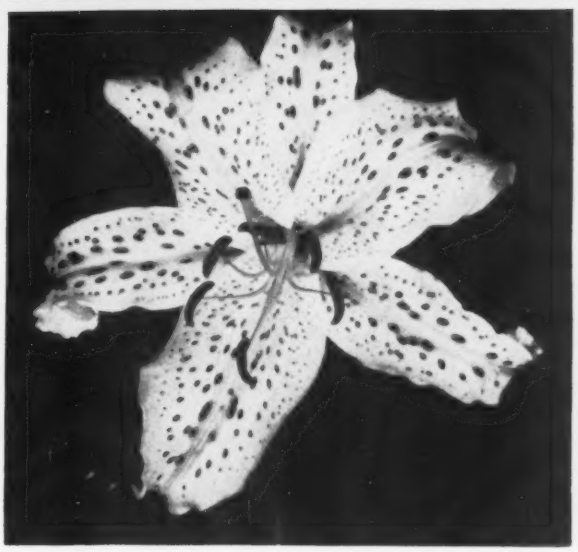
Two different forms of flowers derived from the lily hybrid *L. speciosum* × *L. auratum*.

lilies will replace most of today's species, just as has happened with roses, iris, gladiolus, and many other ornamentals.

By the use of growth regulators we are obtaining selfed seed on lily species heretofore considered self-incompatible, and hybrid seed from some crosses that have never before yielded hybrids. The use of vermiculite as a substitute for soil is enabling us to obtain a high percentage of emergence and survival of lily seedlings, and new fungicides have almost eliminated seedling bulb rots. The hazards of scale propagation have been curtailed by use of the new organic fungicides, and we can now increase new lilies very rapidly. Cytology is contributing to the program by making it possible, by means of differences in chromosome morphology, to determine accurately the parentage of lily species hybrids. As a result, the genus *Lilium* need never be in the position of *Rosa*, in which it is now impossible to determine the ancestry of any modern variety.

Several of our new Easter lily varieties, including one tetraploid, are being increased for us by commercial lily growers. Included in these new varieties are tall-growing lilies for cut flowers, and a short lily for a pot plant. We are also increasing several new types of *L. speciosum* hybrids backcrossed to the *L. speciosum* parent. In addition, there are about a dozen new garden lilies now being propagated for dissemination. By using modern research techniques in lily breeding, we are now accomplishing in a few years what required many years with other ornamental plants.





Two additional forms of lily hybrids from *L. speciosum* × *L. auratum*.

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CONE NEBULA

Internally destroyed and darkly cloaked,
With face averted from our sight,
What primal paroxysm flung you out
Into the frigid star-pocked night?

Was there some ancient, utter struggle,
Some ultimate impact of power,
That hurtled you through exploding space
A million million miles an hour?

And when space folds and light curves back again
Will some observer, mountain-high,
Expose the ravaged pattern of your face
As it comes quietly shrieking by?

Hal Tucker

Evolution of the Quantum Theory

E. U. CONDON

Dr. Condon (Ph.D. California, 1926), who has been director of the National Bureau of Standards since 1945, has just been elected president of the Philosophical Society of Washington, the capital's oldest scientific society. The University of Delhi and the New Mexico School of Mines have recently awarded him honorary D.Sc. degrees, and the Royal Swedish Academy of Engineering Sciences has elected him a corresponding member in recognition of his contributions to science. His article is based on a paper presented in the Section B symposium on "Fifty Years of Quantum Theory" held during the December 1950 meetings of the AAAS in Cleveland, Ohio.

THE great new reformulation of quantum theory, which we know today as quantum mechanics, occurred mostly in 1925 and 1926. I shall not attempt to deal with the events of the past few years, on the ground that it is too soon to try to see them in historical perspective. But so great was the range of subject matter opened up for physics in the quantum-theoretical problems of the period 1925–40, that I shall have difficulty in even taking the barest notice of them in a short article.

It is important at the outset, I think, to stress that, contrary to impressions in some quarters, theoretical physicists are a conservative lot, and do not accept radical revisions of their thinking without a very thorough exploration of all possible alternatives. This conservatism is not the product of intellectual inertia or laziness. I think it is because in classical physics we have inherited an immensely powerful and fruitful tool, as evidenced by its past successes. It is natural to wish to exploit its power to the fullest, not merely because it is something with which we are already familiar, but also because in that way we assure ourselves of building the most unified structure for theoretical physics as a whole.

Therefore, throughout the early period of classical physics, especially in the period before the Bohr atom, leading physicists were most reluctant to go along with the discrete energies and the discontinuous processes that were inherent in the ideas of Planck and Einstein. We have a tendency sometimes to remember the physicists of a previous generation only for their lasting contributions to the physics of our own time. In this way we are likely to ignore the fact that they were quite conscious of basic problems of principle that they themselves were unable to resolve. Since we tend only to publish on those subjects where we think we have a positive result, the literature of this phase of physics tends to be meager, except where personal memoirs and biographical notes are available.

For example, a perusal of Lord Rayleigh's *Scientific Papers,* collected in six large volumes, reveals almost nothing dealing with quantum theory, yet he is known to us for the Rayleigh-Jeans law, which so emphatically requires the quantization of energy in the radiation field. That he was concerned by such questions, however, we see from a brief letter of Rayleigh to Nernst (*Scientific Papers,* Vol. 6, 45), written in 1911, in which he is troubled by failures of the classical theory of equipartition of energy. He writes:

> Perhaps this failure might be invoked in support of the views of Planck and his school that the laws of dynamics (as hitherto understood) cannot be applied to the smallest parts of bodies. But I must confess that I do not like this solution of the puzzle. Of course I have nothing to say against following out the consequences of the [quantum] theory of energy—a procedure which has already in the hands of able men led to some interesting conclusions. But I have difficulty in accepting it as a picture of what actually takes place.

How many of us can echo the feeling expressed in that last sentence! Let me quote him again:

> We do well, I think, to concentrate attention upon the diatomic gaseous molecule. Under the influence of

collisions the molecule freely and rapidly acquires rotation. Why does it not also acquire vibration along the line joining the two atoms? If I rightly understand, the answer of Planck is that in consideration of the stiffness of the union the amount of energy that should be acquired at each collision falls below the minimum possible and that therefore none at all is acquired—an argument which certainly sounds paradoxical.

Nowadays, when every undergraduate physics student learns about critical potential experiments and is taught the experimental facts at once as well as their interpretation in terms of quantized energy levels, it is hard for us to understand what a great step it was to make such a deep-seated revision of fundamental ideas as the quantum theory called for.

The hesitancy did not arise because the new ideals are so difficult to master, once you decide it is necessary to make such a radical break with classical theory; it was rather due to a strong desire to preserve an underlying unity in the structure of physics—just think how foolish all of us would feel if even now someone, by a clever flash of insight, would see a way of incorporating all the phenomena for which we use quantum mechanics into some model based on classical mechanics!

But let us not continue discussing the flavor of the situation without first reviewing a few of the facts about the growth and development of quantum mechanics.

In the few years preceding 1925, the central problem of theoretical physics was that of arriving at a more exact formulation of quantum mechanics than that afforded by the Bohr-Sommerfeld quantization rules and the general guide lines of Bohr's correspondence principle. Bohr himself, who had made the most decisive contributions to atomic theory thus far, was the most insistent critic of the inadequacies of his work—not simply in that it failed to give correct numerical results for special problems such as the ionization potential of helium, but also in that it failed to give a definite basis for calculation of intensity of spectral lines, or a method of dealing with collision problems, or of dealing with chemical bonds and with many other things.

The particular form of classical mechanics known as Hamiltonian was proving most suggestive and powerful in dealing with general questions, and various papers were being written in which tentative trials were being made of ways to modify the Hamiltonian dynamics so as to introduce the discrete energy levels and abrupt quantum jumps so characteristic of quantum physics. A good deal of this was in the nature of probings for a better general treatment of problems that were special enough that there could be, on the one hand, hope that the results could be controlled by specific experimental confirmation but at the same time also general enough to afford guidance for an all-embracing theory.

In this direction the most fruitful step proved to be the development of a quantum theory of dispersion, by adroit modification of the classical theory of perturbation of a conditionally periodic system by the periodic perturbations of the fields of a classical electromagnetic light wave. This was done by Kramers and Heisenberg and led to the association of a quantum-theoretic dipole moment amplitude with each quantum jump, this amplitude being a measure of the strength of the spectral line in emission, absorption, and dispersion. A slight extension led to the theoretical prediction of the Raman effect, but, oddly enough, so far as I know, no one took this seriously enough to look for it. When it was discovered a few years later in India by Sir C. V. Raman it was as a by-product of his intensive studies of light scattering rather than as a result of the theoretical prediction.

The next step was that taken by Heisenberg in proposing to deal with the ensemble of these dipole amplitudes as quantities subject to special mathematical rules of quantum algebra. Immediately it was recognized by his associates, Born and Jordan, that the quantum algebra toward which he was groping was simply the algebra of matrices, and that his quantum rules could be expressed as a particular statement of noncommutation of certain of the matrices involved.

All this was very formal and devoid of direct physical-pictorial content. How much more emphatically would Lord Rayleigh have said to all this: "But I have difficulty in accepting it as a picture of what actually takes place."

The matter was made all the more difficult by the fact that the mathematical theory of matrices was quite unfamiliar to most physicists—for some reason they had not used matrices as a tool for dealing with several topics in classical physics for which they are particularly appropriate. The difficulties of physical understanding were thus enhanced by the need to assimilate an unfamiliar mathematical method.

Most of this was going on in Göttingen, where much of the theory of infinite matrices had been developed in pure mathematics by David Hilbert. There is a story that the founders of matrix mechanics turned to Hilbert for help, and that he advised them that the problem of diagonalizing matrices was always associated with the more familiar problem of finding the allowed values of some

differential equation's boundary value problem. But this tip was not followed up in the excitement of the times, and thus, probably, the Göttingen group missed discovering wave mechanics.

In Paris, a young physicist named Louis de Broglie wrote a doctor's thesis in 1924, which was published in the *Annales de physique,* and to which practically no one in the main stream of theoretical physics paid any attention at the time. De Broglie considered the connection between the wave and particle aspects of light quanta from the point of view of special relativity. The most remarkable suggestion that his studies produced was that perhaps other "particles" than light quanta were somehow associated with wave motions—in particular that the motions of electrons were somehow governed by associated wave motions—and he gave an interpretation of the Bohr-Sommerfeld quantization rules as being the condition that an integral number of these waves be contained inside a periodic orbit.

Shortly thereafter Erwin Schrödinger became acquainted with these ideas on a visit to Paris and found in them the inspiration for his basic papers on wave mechanics, which appeared in the *Annalen der Physik* during the spring of 1926. Also in 1926 there came the discovery by Davisson and Germer, at the Bell Telephone Laboratories, of diffraction of an electron beam scattered by crystals in complete confirmation of the suggested association of a wave motion with problems of electron motion.

Then came the recognition that the wave-mechanical differential equations of Schrödinger were simply an alternative formulation expressing the same mathematical content of the matrices of Heisenberg. The two theories were recognized as simply two representations, or aspects, of the same underlying material, which as yet existed more as mathematical formalism than as clearly formulated physical principles.

By the fall of 1926 Born provided a decisive step in postulating the probability interpretation of the Schrödinger wave function, or, more correctly, the square of its magnitude was interpreted as giving the relative probability of finding the electron in various positions in the physical situation described by a particular wave function. This was necessary in order to give meaning to the formal mathematical calculations by which he gave a wave-mechanical description of the quantum mechanics of collision processes.

Within a year, more general formulations of the transformation theory of quantum mechanics were worked out, principally by P. A. M. Dirac, in England, and by John von Neumann, in Germany. These were natural extensions of the mathematical formalism which included both the matrix-mechanical and the wave-mechanical formulation in a general framework. An essential part of their physical content rested on the adoption of Born's probability interpretation of the wave function.

During the years 1926–27 the principles of the theory became pretty clear, and a host of papers were written applying the new methods to every branch of atomic and molecular theory. Born himself laid the foundations for treating collision problems, Sommerfeld revived the electron theory of metals with appropriate adoption of the Fermi-Dirac statistics for the free electron gas, Heitler and London laid the foundations for the theory of the covalent chemical valence bond, and Heisenberg recognized in exchange interaction of electrons the basis of strong coupling between electron spins, which makes ferromagnetism possible. Many other developments of equal importance could be cited. All these specific results applicable to old phenomena, and some of them giving totally new insights into new phenomena, rapidly gave people confidence in the essential correctness of the new mathematical formalism.

The formalism has been so succcessful, and has continued to be so fruitful in so many parts of atomic, molecular, and nuclear physics, that today it is an indispensable tool for physics. And I think it is fair to say that nearly all the theoretical physicists not only accepted and used the formalism but also adopted the probability interpretation of the wave function.

The general basis of the statistical interpretation of quantum mechanics was, in the fall of 1927, greatly clarified by Heisenberg's analysis of the inherent limitations involved in carrying out certain measurements in the field of atomic physics by the quantum nature of the interactions between the phenomena observed and the observing equipment. This point of view was much more profoundly analyzed and carried much farther in its philosophical implications by the studies of Bohr.

There was one physicist of importance and, so far as I know, only one, who thought deeply on the subject, who did not accept these views as having the fundamental validity that is generally claimed for them. That was, and still is, Albert Einstein. I remember how in 1926–27, when I was studying these things as a National Research Fellow in Göttingen and Munich, we heard that Einstein had refused to accept the statistical interpretation.

"*Der lieber Gott würfelt nicht,*" he remonstrated. ("The good Lord does not shoot craps," to translate into good American.) In this brief sentence Einstein summarized his conviction that a statistical theory of this type could not be more than a temporary makeshift, reflecting an inadequacy of our present knowledge rather than being a fundamental attribute of the world in which we live.

He still holds this conviction, despite the many successes of quantum mechanics in interpreting phenomena, and despite the lack of success attending efforts that he has undoubtedly made in the past twenty years (even though principally occupied with other problems) to provide what to him would be a more satisfactory kind of interpretation of quantum mechanics, or an alternative development that would include the tangible successes of quantum mechanics. He stands pretty much alone in his view, for the general tide of physics has been running strongly in the statistical direction for these past twenty years, but I would not want to say certainly that he is therefore wrong. It could be that future developments might carry us once again in the direction of more specifically deterministic theory—although it is hard to see now any way in which this is likely to come about.

In Born's recent book, *Natural Philosophy of Cause and Chance,* he quotes a letter from Einstein dated December 3, 1947, in which Einstein recognizes that the "statistical interpretation . . . has a considerable content of truth," but he goes on to say that he is "absolutely convinced that one will eventually arrive at a theory in which the objects connected by law are not probabilities, but conceived facts as one took for granted only a short time ago."

With characteristic modesty, however, he concludes this strong affirmation of faith by saying:

Zur Begründung dieser Uberzeugung kann ich aber nicht logische Gründe, sondern nur meinen kleinen Finger als Zeuger beibringen, also keine Autorität, die ausserhalb meiner Haut irgendwelchen Respect einflössen kann.

("However, I cannot provide logical arguments for my conviction, but can only call on my little finger as witness, which cannot claim any authority to be respected outside my own skin.")

For my own part, I have never been repelled by the lack of objective determinism that Einstein finds so unsatisfactory. In the first place, I have always felt very keenly aware that the material we deal with in science is much more subjective than most of us like to admit. It is by no means a complete account in terms of orderly causal relationships of all the sense data we have, but only that part of it which we have thus far been successful in codifying in such terms. It is true that such a tremendous amount of sense data has been codified this way in the past three centuries as to give rise to the hope that all can be so treated. On the other hand, think for a moment how much such data have so far resisted codification in this way.

In terms of such a perspective, how can we have the complete faith that Einstein apparently has in "the perfect rule of law in a world of something objectively existing"? I agree with Einstein that many of the successes of physical theory lend support to this view, but, after all, is it not considerable extrapolation from present knowledge to draw the inference that all of nature must be describable in this way?

Whether all the data of experience can be codified in terms of orderly causal relationships I do not know, of course, but there is no question but that it is useful to have such organization of knowledge carried to its furthest limits, and therefore if an affirmation of faith in the ultimate possibility of doing a complete job stirs us on to greater efforts, I suppose it is a good thing. But this gives to the issue the status of a "pep" talk by the coach in a football game, or any other such rallying cry, which we know from past experience does not always correspond to a critical statement of truth.

If I may stray a little farther in the direction of expressing personal views, I will say that I think the history of science, especially on the metaphysical or philosophical side, has been marred by the tendency human beings have toward facile generalizations, unwarranted extrapolations, and that kind of oversimplification which fails to qualify what would be true if properly qualified. How many pages of disputation have been filled with discussions of free will and determinism! In my view, nothing that physics has to say has any bearing on this one way or the other as an issue about human behavior and human conduct. It was an unwarranted extrapolation in the first place to suppose that we knew so much about classical mechanics, as applied to living tissue, just because we understood the planetary motions fairly well, that we were justified in saying that our knowledge of mechanics strongly supported a view of extreme mechanistic determinism.

This being so, it is, to my mind, equally irrelevant to the basic philosophic issue, when the statistical determinism introduced by quantum mechanics in place of the exact determinism of classical mechanics is used as a basis from which to argue support for the alternative of free will in human behavior. To me it seems simplest to interpret my actual life experiences in terms of a considerable degree of freedom, although the logical possibility

of the alternative view is conceded. But on this general question there is one thing on which I have a profound conviction, and that is that the issue of the deeper interpretation of the meaning of quantum mechanics in the world of atomic physics probably has nothing whatever to do with the issue of free will and determinism as affecting human affairs.

If we now turn our attention to the specific accomplishments of quantum mechanics, it is not possible to separate these from the whole stream of achievement in physics in the past quarter century. Every branch of atomic, molecular, and nuclear physics, including the theory of metals and solids, has been profoundly affected. Quantum mechanics has greatly influenced the development of chemistry, not only in clarifying molecular structure through the interpretation of spectra, but also in interpreting the equilibrium thermodynamical properties of materials by use of the partition function, and also through the use of quantum mechanical theories of chemical rate processes in the hands of Eyring and his associates. Astrophysics has been greatly helped by the more exact theory of atomic spectra and through nuclear reaction rate theory.

It is interesting to note that the entire theoretical structure of nuclear physics is cast in quantum mechanical terms. This branch of physics is so new that it has never been handled any other way. The first application was the interpretation of α-particle natural radioactivity in terms of the barrier leakage, or tunnel effect, as done by Gamow in Göttingen and by Gurney and me in Princeton in 1928.

This same theory of barrier leakage, but applied in reverse to barrier penetration from the outside, made it seem probable that nuclear reactions could be made to occur at lower voltages than would be expected from classical theory. This suggestion became a stimulus in the early 1930s to more rapid progress in this field than might otherwise have occurred. The theory also suggested, as Gurney pointed out, the notion of resonance penetration, making clear why nuclear reactions go more favorably at certain preferred values of energy than at other values either above or below the favored one.

In attempting to build a general theory of nuclear forces between protons and neutrons, quantum mechanics gave the general concept of saturable exchange interaction a meaning that is quite foreign to classical ideas. Relativistic quantum mechanics in the hands of Dirac not only suggested the existence of positrons, but made clear why these particles have only a transient existence, and gave the foundation for calculations concerning the processes by which they are generated and by which they are annihilated. The relativistic theory further gave the basis for the calculation of high-energy Compton scattering processes, as well as pair production processes, which play a decisive role in interpretation of the new physics of the billion electron volt region and higher that is realized in cosmic rays and in the more powerful particle accelerators recently built.

From quantum mechanical theories concerning exchange forces came the concept of Yukawa of these forces having their origin in a field associated with a new kind of particle, the meson. In a short time mesons, too, were discovered, not one, but several kinds, the interrelationship of which with each other and with the other fundamental particles is probably the most important problem of fundamental physics today.

In spite of this long record of remarkable successes, however—and the things here briefly mentioned are by no means all that ought to be discussed—the record is not one of complete and unbroken success. From the very beginning it was recognized that it should be possible to treat the electromagnetic field in a quantum mechanical way in order to give a completely adequate description of such phenomena. The method by which this might be done seemed clear: for many years it had been the custom to describe the dynamics of the electromagnetic field for purposes of statistical mechanics in terms of an analysis into orthogonal wave functions, whose amplitudes were treated as generalized coordinates in the Hamiltonian sense. What was more natural, therefore, than to treat these coordinates as quantum mechanical variables in order to get a quantum mechanics of the electromagnetic field?

This step was taken by Dirac very early, and it had at once a number of successes; among them it gave a correct account of the relation between the spontaneous and induced radiation transition probabilities and a better calculation of dispersion and light scattering. But, when it was applied to more abstruse problems of radiation theory, it was quickly found that the theory often gave infinite results because of the occurrence of divergent integrals in the calculations.

This rather fatal defect has plagued theoretical physics continuously for almost the entire period under review. Throughout the 1930s and early 1940s it was customary, though no one liked the situation, to work problems with this faulty theory, and to try to get reasonable results from it, trying to invent some sort of procedure whereby the range

of integration on divergent integrals would be made finite in order to get finite results. This would have been a distasteful and arbitrary procedure even had it given correct results—which it did not do.

This situation has been to some extent relieved in the past five years by some of the more recondite developments in field theory, especially those associated with the names of Schwinger and Feynman in this country, but this is a topic that cannot properly be reviewed here, other than to say that there is some basis for hope that real progress in quantum electrodynamics is being made.

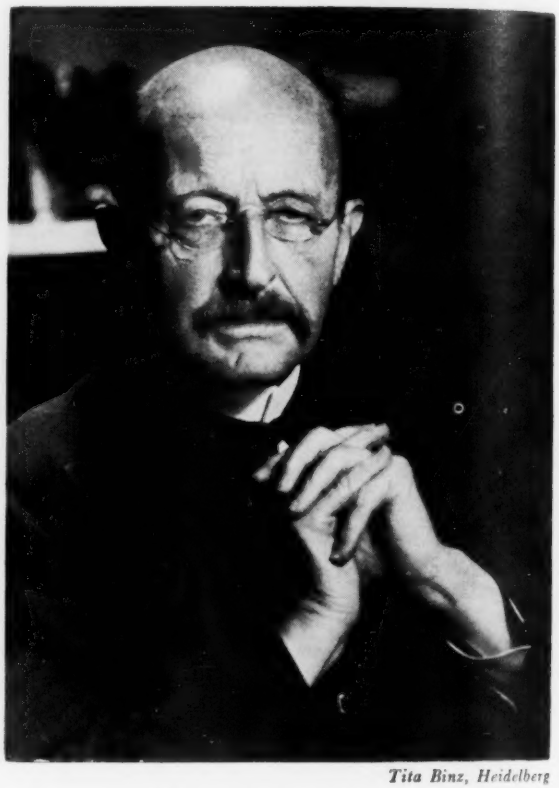
If I were to try to sum up, I think the position today might be stated something like this:

1) Most physicists believe that the Bohr-Heisenberg analysis shows the fundamental basis of the theory to lie in real limitations on the separability of the object world and the effects produced by the observer watching it, and that therefore the statistical nature of quantum theory is basic. But Einstein thinks otherwise, and he might be right in the long run.

2) The nonrelativistic quantum theory for low-energy problems has had so many successes in the simpler problems that a strong feeling exists that all problems of atomic and molecular structure, and even of the crystalline solid state, could be successfully treated this way, except for the great computational labor in dealing with all but the simpler ones.

3) The relativistic theory and the quantum electrodynamics, although beset with difficulties, have enabled approximate treatments of many problems to be given, which are in general accord with the facts and which serve as guide lines around which to organize the experience of high-energy particle physics.

It has been an exciting period of enormous fruitfulness for the development of physics. If the next fifty years bring as much new knowledge as has the half century just passed, then the physics of the year 2000 will be as strange and unforeseeable for us as the physics of today would seem to the physicists of 1900.



Tita Binz, Heidelberg

MAX PLANCK, 1858–1947

Scientific discovery and scientific knowledge have been achieved only by those who have gone in pursuit of them without any practical purpose whatsoever in view.

—Max Planck, *Where is Science Going?* IV (1932)

What Is a Spinor?

F. D. MURNAGHAN

Dr. Murnaghan, who is a native of County Tyrone, Ireland, after twenty years of making mathematics truly the queen of the sciences for students of the Johns Hopkins University Department of Mathematics, has been for the past two years head of the Department of Mathematics of the Instituto Tecnologico de Aeronautica in Rio de Janeiro. His latest appearance in our pages was in July 1950, when he discussed "The Basic Concepts of Calculus."

ALL of us know, at least by hearsay, that theoretical physicists have accomplished wonderful things during the past quarter century and that they have done this by the use of mathematical concepts of an unfamiliar and, supposedly, abstruse nature. We shall try to give in the following brief discussion some idea of what these concepts are and to show that they are wholly natural and not so difficult as they are commonly thought to be.

In our early days we have all studied the concept of a number and have become familiar with the successive extensions of this concept (first the counting numbers 1, 2, 3, . . .; then the integers positive, negative, and zero, 0, ± 1, ± 2, . . .; then the fractions; and, finally, the irrational, or nonfractional, numbers). We have learned that any two numbers, of any one of these successive extensions of the concept of number, may be added together and multiplied to produce two other numbers, their sum and product. The product has the remarkable property that when the two factors coincide the product is never negative; in other words, the square of a number is never negative. At this stage of our training an unfortunate crisis arises. Many teachers say, in effect: "You and I are well aware that a square is never negative. Nevertheless, mathematicians and, more particularly, the book we are using seem to want squares to be negative. Let us, then, suppose that there exists a number i whose square is -1 and let us salve our consciences by terming this number imaginary. We commence, therefore, the study of imaginary numbers; you have my assurance that this study will be profitable." With such an introduction it is not, to me, surprising that many young students (to my mind, the more intelligent) lose interest and self-confidence; they follow obediently the manipulations of the teacher and of the book and pass their examinations but they acquire an inferiority complex concerning mathematics which in many cases stays with them for the rest of their lives. Let us, here and now, remove the mystery concerning imaginary numbers; if we are successful in doing this the road to understanding such modern concepts as spinors, relativistic invariance, etc., lies clear before us.

The real, or nonimaginary, numbers are conveniently represented by the points of a straight line or, better, by the directed segments, or vectors, which connect a fixed point of the line, the origin or zero point, with the various points of the line. It is natural, then, to attempt to represent imaginary numbers (or, more generally, complex numbers which are part real and part imaginary) by the vectors, or directed segments, which connect a fixed point of a plane with the various points of this plane. If, however, we adopt this point of view we see that the two factors of a product play different roles; one factor, the passive factor which is being multiplied, may well be represented as a vector in the plane but the other, the active factor which is doing the multiplication, cannot be conveniently represented as a vector. It is an operator which converts vectors into vectors and is most conveniently represented by two mutually perpendicular and equally long vectors, which figure we may term a square. If these two vectors are of unit magnitude the multiplying complex number simply rotates the vector which represents the passive factor of the product through a certain angle, namely, the angle of inclination of the first of the two vectors of the unit square. If this angle is a complete turn the multiplying complex number is the real number 1 which does not change any number it multiplies; if the angle is a half turn the multiplying complex number is the real number -1 which reverses the direction of every number it multiplies; if the angle is a quarter turn the multiplying complex number is such that two multiplications by it are equivalent to multiplication by -1. This is all that lies behind the mysterious equation $i^2 = -1$, but before the connection becomes clear we must remove the dual role, passive and active, from complex numbers which appear as factors of a product. We do this by replacing each passive complex number (which is represented by a vector) by the active complex number whose representative square has this vector as the first of its two equal and mutually perpen-

dicular vectors. Then multiplication of two complex numbers (now both active) is so defined that the product is the same as we would have obtained if one of the factors were passive. Once this is done it becomes clear that i^2, where i is the square whose angle is a quarter turn, is -1 (where -1 is the square whose angle is a half turn).

It is clear from these remarks that there is an intimate connection between complex numbers and the geometrical concept of a rotation around a fixed line (here perpendicular to the plane in which the passive complex numbers are represented by vectors and the active complex numbers, or operators, by squares); in more technical language, complex numbers are intimately connected with the two-dimensional rotation group. More specifically, the unit complex numbers (i.e., the active complex numbers whose representative squares are formed by vectors of unit magnitude) are represented by the elements of the two-dimensional rotation group; when any passive complex number, or plane vector, is multiplied by a unit complex number of angle A, say, it is rotated through the angle A. There exist, side by side with plane vectors, things of fundamental physical importance which are known as two-component spinors. Like plane vectors, each of these possesses two coordinates or components, but, unlike plane vectors, these coordinates are complex, instead of real, numbers. Thus the geometrical picture of a two-component spinor requires two plane vectors (or passive complex numbers), and these may be of arbitrary lengths and inclined to each other at any angle. When a plane vector is rotated through any angle A the first component vector of the two-component spinor is rotated through the angle $\frac{1}{2}A$, and the second through the angle $-\frac{1}{2}A$. In particular, when A is a complete turn (so that the plane vector is left undisturbed), each of the component vectors of the spinor is reversed in direction. Thus the spinor is able to distinguish between things which to the plane vector look alike; in particular, it is able to distinguish between two opposed directions of rotation of a spinning electron, and it is to this fact that it owes its name. If the spinor is to be left unchanged the vector must be rotated through two complete turns instead of merely one; to amateurs of baseball I might well say that in this kind of baseball the bases must be run twice before scoring.

It is the basic principle of the theory of relativity that the mathematical equations which express the laws of physics, and which require for their formulation the choice of a reference frame, must have a validity independent of the particular reference frame selected. In the days before the importance of spinors was recognized this principle was usually expressed by saying that all physical laws must be formulated in terms of scalars, vectors, and tensors (a tensor being an elaboration of the vector concept and sharing with the latter the property that the bases need only be run once to score). To these we now add the spinor. The simplest conceivable spinor equation (which states merely that the gradient, or space rate of change, of a spinor function of position is zero) is equivalent to the statement that one of the coordinates of the spinor is a function (in the sense of the theory of functions of a complex variable) of the complex position variable, whereas the other coordinate of the spinor is a function of the conjugate complex of this position variable. Thus the theory of two-component spinors is interwoven with the theory of functions of a complex variable, on which the whole theory of the making of maps and so much of engineering is based.

We have pointed out the dominant position occupied by the two-dimensional rotation group in the theory of two-component spinors. The actual theory of modern physics which has been developed by Dirac deals with four-component spinors each of whose coordinates is, again, a passive complex number or plane vector. The group which occupies the dominating position, replacing the two-dimensional rotation group, is now the four-dimensional rotation group or, more precisely, a closely related group known as the restricted four-dimensional Lorentz group, whose definition is intimately connected with the concept of simultaneity in the special theory of relativity. A vector has now four coordinates, three of which are spatial and one temporal, and when a vector is subjected to a transformation of the Lorentz group a four-component spinor is subjected to a transformation in which the halving of angles again comes into play. Any mathematical relation involving scalars, vectors, and spinors which remains valid when the vectors are subjected to a Lorentz transformation, and the spinors to the associated spinor transformation, is said to be relativistically invariant and, in accordance with the fundamental postulate of the theory of relativity, only relativistically invariant relations are legitimate expressions of the laws of physics. Dirac's equations for an electron in an electromagnetic field are the simplest relativistically invariant relations, connecting the space-time rate of change of a spinor function of space and time with the electromagnetic potential vector and the scalar rest mass, which assure the conservation of electric charge. For a force-free field and negligible mass they possess a striking similarity to the Cauchy-Riemann equations of the theory of functions of a complex variable.

The Application of Ecological Knowledge to Forestry

HENRY I. BALDWIN

The author (Ph.D., Yale, 1931) is research forester of the New Hampshire Forestry and Recreation Commission and has resided on the Fox State Forest in Hillsboro, New Hampshire, since 1933. He was previously connected with the Brown Company, Berlin, New Hampshire, and has taught at Penn State College. He is chairman of the Committee on Study of Plant and Animal Communities of the Ecological Society of America. His article is based on a paper presented in the symposium on "The Application of Ecological Knowledge to Human Affairs" at the September 1950 meeting of the Ecological Society.

FORESTRY is a subject that can justly lay claim to having applied ecological knowledge to a high degree. Forestry and forest ecology are closely allied in the minds of many people. Certainly all foresters are deeply indebted to ecologists, who were the early students of forests, the plant communities that demonstrated so well many of the fundamental concepts of ecology. Ecology was early recognized as the foundation upon which the science and art of silviculture must be based.[1, 2]

It would be a formidable task, indeed, to review all the applications of ecology to forestry in its various ramifications, or even to evaluate the most important ones, and still more difficult to condense them within the limits of this article. The purpose will be, rather, to cite some examples that may be suggestive of the dependence of forestry upon ecology, both successful applications of ecological knowledge and the unfortunate results where it has been disregarded.

Before forest management can begin there must be information on the location, extent, and composition of forests. The associations of the ecologist form a basis for forest types, a delineation of which is essential to the forester. Not only must the existing composition of the forest be determined, and classified by types, but a thorough understanding of succession is also necessary; for mere inventory purposes, "cover types" may suffice, but future management requires an estimate of their potentiality as well. Aspen is a temporary type that may in a few years become one of several later stages in succession, whereas in sugar maple–basswood, the successional change, if any, is imperceptible during any one lifetime. A true insight into any given forest stand, and what its development can be expected to be, either with or without alteration by man, can be gained only by taking account, not only of its present composition, but also of the subordinate flora, the humus, and the underlying soil. Where volunteer tree reproduction is present, this may serve as an indication of what the next crop should be. Before the New England hurricane of 1938 old field white pine stands that had originated on abandoned fields on deep, moist, loamy soils through the selective action of cattle grazing (whereby hardwood competition was eliminated) developed dense understories of white ash, sugar maple, and other hardwoods. The hurricane (or current logging operations) accelerated succession, and efforts to perpetuate pine on such sites appear doomed to failure.

Students of the uniform pine forests of northern Europe noted that the lesser, or surface, vegetation varied with soil and moisture conditions and could be used as an indicator of site quality even in young stands, or in their absence. The work of von Kruedener[3] and of Cajander[4] and his pupils has its counterpart in the extensive investigations of indicator plants in America. Although such methods work best in fairly simple forests of few species (such as Scotch pine in Finland or jack pine–black spruce in Canada) the principle of indicator vegetation can be applied in a broad way almost universally, for often the flora will describe the site in a subtle integration of hard-to-measure site factors that serve a useful purpose. Thus blight-

Fire-killed timber, Marlow, New Hampshire, 1942. Succession following fire on this area will be aspen, gray birch, and red maple.

resistant chestnut trials have been established in a wide variety of areas east of the Mississippi on the basis of sites where maidenhair fern (*Adiantum pedatum*), spice bush (*Benzoin aestivale*), and jack-in-the-pulpit (*Arisaema triphyllum*) were present. About twenty-five years ago the writer classified sites in northern New England according to their difficulty in reproducing to spruce and fir by the indicator vegetation, and this has since been confirmed by other investigators.

One of the chief tasks of the silviculturist is the natural regeneration of forests, that is, carrying out cutting operations so that the commercially desirable species best suited to the site are provided with optimal conditions for seed production, germination, and survival. The care necessary to achieve this varies widely, and often from acre to acre. Only a keen observer of successional trends can do this. Many attempts to provide reproduction by leaving scattered seed trees failed because of inadequate seed supply, too dry, hot, and exposed seedbed, or because rodents and birds consumed a large proportion of the seed. Competition by other tree species than the desired crop and by other plants is a serious obstacle. Cutting in Northern hardwoods–spruce forests is frequently followed by inroads of mountain maple (*Acer spicatum*) and witch hobble (*Viburnum alnifolium*), to the exclusion of spruce and fir and, to a lesser extent, valuable hardwoods. Large clear-cut areas alter the microclimate significantly. Hollows become frost pockets where late frosts retard growth. Temperature extremes are increased everywhere.

A knowledge of ecology is also indispensable in planning and carrying on reforestation by seeding or planting. Direct seeding, successful in Scandinavia, has until recent years been a complete failure in the United States because of disregard of many ecological factors. At first broadcasting seed like grain appeared to be an easy and logical way to establish forests. After all, it was nature's method. Rodents destroyed a large part of such seed, and only rarely did the limited quantity of seed find a suitable place for germination. A study of the sites disclosed that only north exposures close to rocks and logs retained moisture or were safe from lethal surface temperatures.

Tree planting in the Great Plains and prairie regions has been attempted since the time of settlement, but earlier mistakes were overcome only after site conditions became better understood as a result of ecological investigations. The beneficial effects of shelter belts are also better known today.

Planting conifers on cutover land where the soil favors tolerant hardwoods has been a common mistake. A study of soils and indicator plants should show where hardwood growth is likely to be so vigorous that only by repeated costly weeding can conifers be kept from suppression, and then only temporarily.

The choice of planting stock with special reference to the source of seed is a subject bordering on both genetics and ecology. As early as the nine-

teenth century it was noted that trees grown from seed from foreign sources made inferior growth, or the trees developed abnormally compared to those grown from local seed. These observations gave rise to the concept of local races, or ecotypes. The use of seed from sources unsuited to the planting site has caused discouragement and has been costly in time and money. The full extent of this loss is unknown, because not all the reduction in growth rate and susceptibility to insects and disease, snow break, etc., are manifest, or can be laid certainly at the door of provenance. Further, no one ecotype forms a homogeneous population in its native habitat, the best adapted individuals constantly tending to emerge as dominant. By cultural treatment man, in his obsession with the phenotype, has unwittingly perpetuated unsuitable strains. Thus some ostensibly indigenous Norway spruce stands in Germany are composed of some early-sprouting individuals and some late-sprouting. Left alone, the early-sprouting forms would be suppressed by being repeatedly killed back by frost, but thinning operations have allowed them to survive along with the late sprouters. The whole subject of exotics, whether in forestry or horticulture, is one on which the only sound basis for success is thorough study of the ecology of the native habitat.

Appreciation of the ecological role of forests is evidenced by the large number of investigations that can be classed under "forest influences," defined "as including all effects resulting from the presence of forest or brush upon climate, soil water runoff, stream flow, floods, erosion and soil productivity. Health and economic conditions have been included. . . ."[5] Even a brief enumeration of the actions and reactions of the forest and environment, as suggested by this definition, would be beyond our scope, but the ecological implications are profound, and the sins of omission and commission have led to disastrous results. Floods, erosion, and famine in China have been linked with deforestation. However climatic changes may have been involved in this oft-quoted instance, the small-scale examples of erosion and runoff following denudation of forest cover on steep hillsides are easily observed, as are the beneficial effects of shelter belts on humidity and evaporation rate. The higher infiltration capacity of soils under high forest has been fully demonstrated.

Forest protection, including protection from fire, wind, insects, diseases, mammals, and birds and miscellaneous agencies, is becoming more and more recognized as an ecological problem. Forest fires are obviously influenced by weather, fuel type, and condition, and by slope, forest type, and many

Natural reseeding of white pine. Fox State Forest, Hillsboro, New Hampshire.

Chaparral forest on foothills, Mount Egmont.

other factors. The integration of these factors for the prediction of forest fire hazard has drawn fully on ecological knowledge of forest types. The importance of the biotic factor, man, as a cause of fires involves human ecology. The psychological warfare on man-caused fires illustrates this. The forest fire control technician appraises not only the weather, but also the composition of the forest and the subordinate vegetation and its seasonal aspect, and even the character of the soil. These and other factors influence the kind and amount of fuel, and their drying rates. The technician also takes into consideration the number of people who may be in the woods.

The control of harmful forest insects requires a knowledge not only of the ecology of the insect, but of the forest environment. Temperature tolerances of some forest insects are illustrations. The European pine shoot moth (*Rhyacionia buoliana*) does not survive winter temperatures of – 10° F. to – 20° F. and hence need not be feared where such minima are normal. The fir bark louse (*Adelges piceae*) and many other scale insects suffer heavy mortality from low winter temperatures. A study of temperatures in the forest might indicate the places where such thermal control could be counted upon to be adequate. Much, obviously, is still to be learned of the ecological factors that influence outbreaks, and when the former are well understood, we may have an important aid in forest insect control.

The white pine weevil (*Pissodes strobi*) is a native forest insect, termed by the U. S. Bureau of Entomology and Plant Quarantine the most important destructive forest insect in the Northeast. Its ecology is still imperfectly understood. It was long ago observed, however, that it attacked trees growing in the open more often than those growing in the shade. The chief recommendation for control still is to maintain a partial canopy of other trees over young pine and spruce until these reach a height where the weevil is less injurious, or, where bare land is to be reforested, to establish a nurse crop first. In spite of this knowledge of the ecology of the insect, thousands of acres of white pine, Norway spruce, and other susceptible species have been planted in the open, soon to become mere bushes because of weevil attack.

Examples of ecological control of forest diseases are also many—the use of fire in controlling *Septoria* needle blight of southern pine, to name but one. Where alternate hosts are involved, as in white pine blister rust, the ecology of *Ribes*, its typical habitats, factors influencing regeneration, and seasonal aspect as affecting ease of eradication all are worthy of consideration. Many species of

Ribes, for instance, leave out early in the spring before associated plants. Where diseases are spread by insect vectors the ecological background of insect, disease, and host plant may provide a framework for control.

There are many baffling problems in forest protection where the direct causative agent is not apparent. The so-called dieback of birch in the Northeast is a case in point. In such problems a broad ecological study is the obvious course. Thus Hawboldt[6] has shown that some fungi associated with the roots of dying birch become parasitic only with reduced moisture supply. Other investigations of the environment are expected to throw light on the causes of excessive mortality in birch.

An outstanding case of relationship between animal ecology and forestry is the browsing of tree seedlings by deer, which was recently surveyed in Wisconsin.

The utilization of forest products might seem a far cry from ecology, yet investigations have shown that physical characteristics of woods are in some degree dependent upon the soils, the climate, or the exposure under which the trees grew. Such conditions as compression wood, percentage of summer wood, presence of wind shake, and frost cracks are related to site factors. The season and the weather still exert a great influence on the harvesting, extraction, and manufacture of many forest products.

To summarize, ecology may well be termed the foundation not only of silviculture, but of wellnigh all other branches of forestry. Many ecological bases have long been recognized in folklore and practiced for generations without our fully appreciating why. The science of ecology has provided the answer. Thus ecology should furnish a guidepost for foresters to enable them to foresee the consequences of forestry operations that alter the environment.

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Wildlife Management and Ecology

GUSTAV A. SWANSON

Although his training was in zoology (Ph.D., Minnesota, 1937), Dr. Swanson's professional work has been entirely in the field of wildlife management. He resigned his post as chief of the Branch of Wildlife Research, U. S. Fish and Wildlife Service, in 1948 to become head of the newly organized Department of Conservation at Cornell's State College of Agriculture. He is also editor of the Journal of Wildlife Management. *His article is based on a paper presented in the Ecological Society symposium on "The Application of Ecological Knowledge to Human Affairs," held during the Columbus meeting of the A. I. B. S.*

OPINIONS might vary as to the date when wildlife management began in the United States, but few would quarrel with the statement that over 90 per cent of its growth and development has occurred in the past twenty years. Nor would many dispute the statement that most of this development has occurred through the recognition and application of ecological principles and techniques. Few, if any, individuals played as important a role as Aldo Leopold in influencing the growth of American wildlife management along scientific lines. His text, *Game Management,* published only seventeen years ago, was essentially a treatise in applied ecology.

In trying to review the field of wildlife management today and the role which ecology has played in its development, I have tried to visualize its status twenty years ago and contrast it with the present. Evidences of progress are easily seen. Among the more obvious are the number of trained men employed in the field, which in twenty years has grown from dozens to thousands; the establishment of a professional society and its own journal; and the increasing number of American colleges and universities endeavoring to train professionals in the field, now more than thirty.

Reasons for this growth are not quite so obvious. The stage was being set by the decline in numbers of many important species of wildlife, and the maturing of America from a pioneer stage to a society with fewer frontiers to exploit, and hence with more interest in conservation. Then, too, ecology itself was maturing rapidly during this period, the closely related field of forestry was also forging ahead, and both have had due influence on the development of wildlife management. If I were asked to designate more specifically contributions or events that have hastened the growth of wildlife mangement in this country, I should settle, I believe, upon the contributions of Aldo Leopold during the late twenties and early thirties, culminating in the publication of his text, and upon the enactment of the Federal Aid to Wildlife Restoration (Pittman-Robertson) Act in 1937, which has made it possible for the state game and fish departments to employ trained personnel and tackle many wildlife management projects which previous lack of sufficient funds did not permit.

In game management, Leopold recognized five main types of controls: control of the take by hunting; control of predators; the establishment of refuges and other reserved lands; artificial stocking with animals trapped elsewhere or reared in confinement; and, finally, improvement of the habitat. Twenty years ago the emphasis was overwhelmingly upon restrictions of the take, predator control (mostly through payment of bounties), and artificial stocking, usually from game farms. During the past two decades the recognition and application of ecological knowledge have resulted in deemphasizing the importance of predator control and artificial stocking, and in increasing recognition of the importance of habitat improvement. The role of hunting restrictions is still as important as it was two decades ago, because of the greatly increased demands being placed upon our stock of wildlife by an increasing number of sportsmen, with increased leisure and greater mobility through

modern means of transportation. The acquisition of lands especially for wildlife has also progressed far during the period, but there is much greater emphasis now upon improving the habitat thus acquired rather than merely designating it as wildlife lands, which was usual twenty years ago.

In the brief space at my disposal I should like to cite a few specific examples of the way in which ecological knowledge has been applied in the field of wildlife management.

Estimating with reasonable accuracy the populations of wild animals is essential in all phases of wildlife management, and the development of wildlife census or inventory methods has been one of the major objectives of wildlife research during the past two decades. Estimating accurately the size of populations of vertebrate animals is difficult at best, but wildlife managers are faced with particularly acute problems in this respect. It is seldom possible to employ the precise and intensive methods of the research ecologist, for obvious reasons. The area to be covered may be as large as an entire state, the period available for the estimate may be very short, and the deadline date for the final results may be determined by administrative requirements, rather than by the habits or biology of the animal in question. But much progress has been made, though still more remains to be done.

Twenty years ago hunting seasons were set almost entirely by legislative edict, determined many months or even years in advance of any given hunting season, and ignoring completely the rapid fluctuations in game populations which may and do occur. Today the Fish and Wildlife Service and most of the states are attempting, at least, to make annual inventories of their wildlife populations as a basis for the annual hunting regulations. All too frequently legal restrictions or traditions require that a year's hunting season be determined long before it is really possible to determine the size of the season's production. In many, if not most, states the biologist has been expected by May or June to furnish the information upon which the following fall hunting season would be based. The administrator's stock answer was that waiting until the season's production could be measured more accurately would make it impossible to carry the regulations through the devious administrative channels required before they become legally effective—consideration and approval by the Commission, the legal counsel, the Governor, and finally the printing and distribution to the sportsmen, the ultimate consumers.

Such legal red tape has been particularly annoying to the wildlife manager as his methods of measuring the game populations became more accurate. Seldom, however, has it been broken. It is particularly gratifying, therefore, to note that last year for the first time the Federal Fish and Wildlife Service delayed the final determination of the migratory bird seasons in the United States until after August 1, when much of the information which could be gathered on hatch and survival of broods was actually available for consideration at the time when the hunting seasons were determined. I would consider this a signal victory of wildlife ecology over administrative red tape.

Just one more example from the field of wildlife censuses will suffice. Missouri's wildlife men have been outstanding in recognizing the effect of soils upon numbers and general condition of wildlife, and the quail census method developed by the late Rudolf Bennitt employed this general principle in attaining greater accuracy. It was discovered that the different soil types, broadly speaking, had different levels of game production, just as they did of the production of agricultural crops. In measuring the trend of the quail population on a statewide basis, it was possible to attain greatest accuracy by designing the census routes so as to sample each of these broad soil types adequately, and analyzing the data by soil types rather than by political units, as was previously the custom.

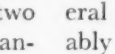
Carrying capacity is an ecological concept which is now well recognized by professional wildlife managers, but the general public, which must register its approval before many wildlife management measures can be adopted, has found it most difficult to understand. One very widespread result of this failure in general conservation education has been the development during the past ten or fifteen years in many regions of the United States of populations of big game which greatly exceed the carrying capacity of their range, and which have brought with them many of the unfortunate results of such excessive populations—destruction of the preferred foods on the range and decline of its carrying capacity; waste of much of the game population through starvation; deterioration of the quality, size, and reproductive rate of the herds through malnutrition; and excessive conflict with other human interests, such as agriculture, horticulture, and forestry.

The fields of hunting and fishing excite such high public interest that the administrator of wildlife resources must be responsive to public opinion if he is to remain long in office. He is frequently in the position of knowing what it would be proper to do, but being prevented from doing so by public

opinion. This problem occurs in its most acute form in the management of deer and other ungulates. Underutilization of big game populations in the United States, permitting them to increase beyond the carrying capacity of their range, has become virtually characteristic of American game management. This striking failure is not due to inadequate investigations or ecological thinking by the professional game manager, but rather to an abject failure of the machinery for educating the American public. Perhaps our American public, now preponderantly urban, is a particularly difficult one to educate in ecological matters. In the field of wildlife management, however, it must be recognized that, difficult or not, the job must be done. Unless at least that portion of the public which is vitally interested in wildlife can be persuaded to absorb a few of the more elementary ecological concepts, it will always stand as a serious stumbling block in the path of wildlife management.

In the field of refuge management, particularly for migratory waterfowl, there seems to be another serious problem in the making. For years it has been accepted without question that an adequate system of waterfowl refuges throughout the United States on which the birds could rest, feed, and in some areas breed without disturbance was essential to maintaining a satisfactory population of waterfowl in North America. The Fish and Wildlife Service and the majority of the states have spent many millions of dollars acquiring, developing, and maintaining waterfowl refuges during the past two decades. The role of the refuge in waterfowl management is now coming under closer scrutiny. The blind assumption that prohibiting hunting on a few thousand acres will help save the ducks is giving way to serious study. It is becoming increasingly apparent that the establishment of a protected area may, in many cases, hold large populations of waterfowl in a region where they are far more vulnerable to hunting than if the refuge did not exist and the birds went on to wintering areas in the Gulf states or Mexico, where hunting pressure is less severe. It is being asked with increasing frequency whether a projected refuge may be a death trap instead of a sanctuary. Horseshoe Lake in southern Illinois, where a waterfowl refuge of some 3,000 acres established by the state resulted in the most concentrated slaughter of Canada geese on record anywhere, is probably the most extreme case. There are, however, many other instances where the establishment of a protected area increased, rather than decreased, the hunting pressure on the population of birds using it. This is just one of numerous examples that could be cited of a problem in wildlife management which will yield only to the best ecological approach.

I should say, then, that the very real progress that has been made in the field of wildlife management in America during the past twenty years has been due in large part to the employment of ecological data, concepts, and techniques. The professional wildlife manager, with admittedly still far to go, has embraced the ecological method to excellent advantage. On the other hand, the general public, which so frequently must be reasonably well informed before it will permit the employment of sound management practices, has found it difficult to understand ecological concepts.



To look at his picture as a whole, a painter requires distance; and to judge of the total scientific achievement of any age, the standpoint of a succeeding age is desirable.

—John Tyndall, "Science and Man," *Fragments of Science*

The International Map of the World

DEAN S. RUGG

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MAP coverage of individual countries has varied with the size and advancement of the particular state. In most countries, however, there has usually been a gap between small-scale general maps and detailed topographic sheets, and a great amount of nonuniform cartographical material has existed. About 1890 there arose a demand for a uniform map of the world on a scale that would make such a project practical and at the same time fill the gap in scale coverage. The project was visualized as an atlas, on the scale of 1: 1,000,000, representing a compilation of all existing cartographical data. This resulted in the International Map of the World being begun in 1909; at the outbreak of World War II it was approximately a third of the way toward completion.

Today, following the war, the project is even more significant with the growth of the One World concept. The value of the map as reference material, as well as a base map for isolated areas where there is nothing else available, is well known. It is also an example of successful international cooperation. Unfortunately very little progress is being made. The offices of the Central Bureau of the International Map at Southampton, England, were destroyed by bombing during the war, and this chief centralizing force is at present reorganizing. In the various countries of the world where work was going on before 1939, new problems of a political and economic nature have pushed the map project into the background.

The latest (1938) Report of Published Sheets by the Central Bureau lists approximately one third of the 974 sheets necessary to cover the land surface of the world. The greatest gaps in the completion of the series are in central and eastern Asia, North America, and Australia. It is important not to lose the work done on the International Map, and at the same time it is hoped that work can progress at an even faster rate on new sheets. Since the concept of the map had its start in the International Geographical Congress, it is logical that future plans in regard to the project be considered at meetings of the congress.

Organization and Development of the Project

The desirability of a world map on a uniform scale and conforming to a standard scheme of symbolization and reproduction was first brought to official international attention in 1891, at the International Geographical Congress held at Bern, Switzerland. Dr. Albrecht Penck, then professor of geography at the University of Vienna, presented a paper on the importance of a uniform world map on the scale of 1: 1,000,000.

This paper stressed the fact that, although much of the earth had been mapped on varying scales, circumstances tended to reduce the value of such a great mass of cartographic data. The more significant of these were the lack of uniformity as to scale, projection, and style of execution; the widely scattered places of publication; and, finally, the nonavailability of the maps because of limited publication. Thus, Dr. Penck, the "Father of the International Map," believed that the compilation of all existing cartographical material and its condensation into an atlas of the world would have a practical as well as a scientific value.

It is in this statement, emphasizing the desirability of such an atlas, that we find the underlying purpose behind the undertaking. According to Dr. Penck, the gap that exists between the small-scale maps of atlases and the large-scale topographic series of most countries could be bridged by the development of such a map on the scale of 1: 1,000,000. This scale would make the project possible and still be large enough to show most of the existing cartographical information. It was not intended as one large map wherein the sheets could be joined, but rather as a uniform atlas of the world on the largest practical scale.

In 1893 many distinguished German geographers and scientists met at Stuttgart, where the question was again seriously discussed. After this the question was taken up by several geographical societies, especially those of Paris and London, and the matter was fully discussed at the meeting of the International Geographical Congress in London in 1895. In certain countries the preparation of maps

on the scale of 1: 1,000,000 had begun. These maps were chiefly of areas in Asia and Africa, and, though alike in size of sheet and scale, they lacked uniformity in other details.

There was further discussion of the map at the International Geographical Congresses in 1899 and 1904, but no generally accepted scheme could be found because of various national differences of opinion, notably the reluctance of the British geographers to accept the metric system for the map and the insistence of the French geographers that the meridian of origin be that of Paris rather than that of Greenwich. In addition, the lack of government sanction or other means of initiative within the separate countries held back the growth of the project. Henry Gannett, geographer for the United States Geological Survey, was primarily responsible for bringing the discussions to a head. His proposal at the 1904 congress that an international map on the scale of 1: 1,000,000 be definitely standardized was unanimously accepted by the International Geographical Congress at Geneva in 1908. That organization passed a series of resolutions dealing with the style and symbolism of the map, and other matters connected with its production.

Because of the unofficial nature of the proceedings, it was considered desirable that one of the governments interested should invite official delegates of the other countries to meet in conference and to arrive at an agreement that they would be able to submit to their respective governments for approval. The British government took the lead and invited an International Map Committee, composed of delegates of the governments with representatives of ambassadorial rank at the Court of St. James's and also of Canada and Australia, to meet in London. It is from the year 1909, when this meeting was held, that the International Map is generally considered as dating. Resolutions were adopted that defined precisely the scale and projection of the map, the scheme of symbolization, the classification of natural and man-made features, the spelling of names, and the layout of sheets. There was no mention at the meeting that the map should be a truly international project to which all the countries of the world would contribute the sheets covering their own territories. Supplementary resolutions (which were never formally signed) stated that production of the map should be apportioned among a few countries and could be carried out by either government bureaus or private cartographical establishments.

The Second International Map Conference was held in Paris in 1913, and was attended by representatives of thirty-five governments. At this meeting the map was finally made an international project and was called Carte International du Monde au Millionième. One of the resolutions stated that "each State which possesses a suitable cartographical establishment shall prepare and publish the sheets which include the whole or part of its territories." No resolutions were made at the time concerning countries with no cartographical establishment or regarding the coverage of areas involving nonmembers of the conference. It was also agreed to establish at the Ordnance Survey Office in Southampton, England, a Central Bureau maintained by international subscription and charged with the duty of coordinating the work of the various nations.

The first world war broke out soon after the Paris conference, and the Central Bureau was not able to function during that period. In 1914 only eight European sheets of the International Map had been produced. The Allies expressed a need for an accurate general map of all of Europe in the style of the International Map, and the Royal Geographical Society set about compiling, under the direction of the British General Staff, a series of European substitute sheets for the International Map of the World. These maps, although not conforming exactly to the Paris Resolutions of 1913, filled a wartime need and served as a base for international postwar deliberations and agreements, thus demonstrating one of the advantages of a world map on a uniform scale.

The period from 1918 to 1939 witnessed the development of the Central Bureau. This body was a purely volunteer organization and had no administrative powers enforceable by international agreement. Its original functions were to coordinate the work of the various offices engaged in production and to record the progress made by the whole project. Other subsidiary functions were to supply information about the map to the public and to producing offices, as well as to criticize sheets in the proof stage. The only distributing function of the bureau has been to send out to the various adhering countries the sixty copies of each published sheet which are required of all member-countries. The Central Bureau has never attempted to act as an agent for the sale of individual sheets; consequently, the procurement of these sheets has been a problem that has never been satisfactorily solved.

The development of the International Map in the years following the first world war was characterized by slow progress in the production of sheets. Most countries were occupied by domestic

problems, and international projects received little support. There were no important map conferences held during this period (1918–39), and questions that came up were of a specific and technical nature and were handled through Central Bureau correspondence.

One of the greatest hindrances to the progress of the International Map has been the inactivity of the United States. This country was one of the early leaders in discussions for the carrying through of the map and could well have sanctioned the completion of sheets covering the area. The failure of any one map agency to have clear-cut administrative and operating responsibilities to represent the United States was perhaps a dominant reason for the lack of participation. The Department of State is directly concerned with all international issues for the United States, but it does little mapping. The two primary mapping agencies of the United States before World War II, the Geological Survey of the Department of Interior and the Coast and Geodetic Survey of the Department of Commerce, have been occupied with large-scale topographic mapping and surveying. The Army Map Service, established during the second world war, and one of the largest governmental mapping agencies in the world, is admirably equipped to undertake any cartographic project.

A private institution, the American Geographical Society, did enter the field of the International Map, although the original purpose did not bear any relation to this project. The Map of Hispanic America on the scale of 1: 1,000,000 was completed in 1945. There are 107 sheets of this map, which covers all the land areas of the American continent from the Mexican-United States boundary to Cape Horn, including the West Indies. The scale and scheme, with certain modifications, of the International Map were chosen for the Hispanic project. The Geographical Society felt that a map covering so large a section of the earth's surface should be produced in a form that would fit into the scheme of the International Map and serve in connection with it until such time as the Spanish-American countries were in a position to produce their own sheets.

Technical Aspects of the Project

The standard sheets of the International Map embrace six degrees of longitude by four degrees of latitude. The map surface, neglecting margins, is thus about twenty inches by seventeen inches near the equator but naturally becomes smaller as the meridians converge toward the poles. North of 60° N or south of 60° S two or more sheets may be combined to form coverage over 12°, 18°, or 24° of longitude. The polar regions are to be treated separately. The Greenwich meridian is used as the point of origin for longitude. The projection is a modified polyconic, and, although each sheet fits its four marginal sheets, no more than five sheets in the form of a cross can be put together. The conventional signs and symbols are standard and do not differ notably from those of standard topographical sheets. Absolute elevation is shown by the use of contour lines, and colored tints help to emphasize the relative relief. The contour interval is changed according to the relative relief of the area, being small in areas of slight relief and large in mountainous regions. Hydrographic features are in blue, and ocean depths are emphasized by tints and contours. The graphic scale is given in kilometers and miles, and all marginal information is in the language of the producing country and also in either French or English. This includes index diagrams, reliability diagrams, legend, and compiling sources, as well as date of publication. Sheets are indexed as shown on the Index Map of Coverage, and the name of each individual sheet usually refers to a prominent town or feature of the area.

Although most of the technical aspects connected with the IMW are covered by resolutions, the international nature of the map has brought up various problems that have never been completely solved. Basic to a solution of these problems is the need for international cooperation between states. This is best illustrated by the case of Europe. Here, as a result of the small size of individual states, sheets will cover more than one sovereign area; hence, cooperation is essential. In the past, lack of cooperation has been responsible for duplication of standard sheets, as well as publication of series of sheets for all of Europe by a single agency. The United Kingdom, Germany, Italy, and other countries have compiled IMW sets for most of Europe, for these states were unwilling to be dependent on one another for sheets covering Europe. This duplication of effort shows that in Europe the IMW has not been the best example of sound international cooperation. In cases where such cooperation has existed, and prescribed standards have been followed, however, it has been found that a better map is produced than one compiled by a country located some distance away without access to primary source material.

Specific problems that need to be solved if the International Map of the World is to become the world reference atlas are:

1) Improve cooperation between countries so

that the exchange of cartographic material may be facilitated. The controlling agency of the future must prescribe the official sheet allocation (where several states are found within one sheet) and coordinate the exchange of cartographic material so that the best possible sheet will result. Furthermore, a central distributor for the sale of sheets should be established so that the problem of sheet procurement can be solved.

2) Improve the uniformity of sheet composition. The amount of detail on a sheet should be related to the area's stage of economic development. From present sheets one might get the idea that there are more first-class roads (per square mile) in Finland than in Belgium. A possible solution might be the division of the world into areas of equal technological development and the establishment of cartographic standards for each of these areas.

3) Recognize the lack of uniformity as to the existence of cartographic material throughout the world. An effort should be made to increase the surveying knowledge throughout the lesser known parts of the world. The use of aerial photographs and the development of photogrammetry will help.

In addition, the problem of languages has been a difficult one. No uniform procedure has been followed in the past—so that sheets differ widely in the use of place names. In general, the Roman alphabet is accepted as standard, and countries not using this alphabet, or lacking an alphabet, have produced Romanized editions in addition to their own national sheets. In many cases where the alphabets differ, no special edition is produced, but a system of transliteration is included. Approved transcription systems are followed in the case of nonalphabet countries. Resolutions have stated that the spelling of place names was to be that adopted by the individual country, or in the case of colonies, that of the governing power. The many political changes since the resolutions were originally promulgated, as well as the nonuniform approach to languages and place names, would indicate a need for a review of this problem.

Analysis of Present Status of Publication

The basis for this analysis is the official reports of the Central Bureau. Only the sheets listed in these reports have been included, with the few exceptions of sheets known to have been published during or following World War II. In addition, the sheets listed as "War Sheets" were all produced during the last war by war agencies and not as official contributions to the IMW. The War Sheet totals are included, since many of these sheets cover areas that were not mapped as such under the IMW project. These War Sheets could be used as aids in producing or revising future official contributions to the International Map of the World.

The last Report of the Central Bureau (1938) listed a total of 397 published sheets of the International Map. Actually, however, this number includes some duplications, as well as only 69 sheets for the Map of Hispanic America. The latter has since been completed and comprises 107 sheets. With the subtraction of duplications and the addition of new sheets published since 1939, the totals are as follows:

974 separate sheet areas required to cover the continental land masses of the earth and the larger islands, exclusive of the polar regions and small islands in the Pacific and Indian Oceans.

387 different sheet areas covered by official IMW standard and provisional (lacking in some respect in the qualities required for an accepted sheet of the International Map) sheets; 248 of these are standard and include the Map of Hispanic America.

658 different sheet areas were published as standard, provisional, or "War Sheets;" thus 261 separate sheet areas covered by war mapping on the scale of 1: 1,000,000 had not been mapped by either standard or provisional IMW sheets.

There are 49 adhering countries listed in the last Report of the Central Bureau, five of which were at the time listed as colonies. Included in this total are some states now behind the Iron Curtain, whose future status is uncertain. Of the adhering nations, it is notable that only 28 had published one or more sheets. The Soviet Union has never been an adhering country. However, Russian sheets on the scale of 1: 1,000,000, conforming to the layout of the International Map, which were produced both before and during World War II, would appear from descriptions to be acceptable as standard sheets. Of the Hispanic American countries only Argentina and Brazil were listed in the last report as contributing any sheets to the International Map.

The more apparent of the present gaps in the International Map is the continent of North America, which includes the United States. Of the estimated 70 sheets necessary to cover the latter country and its possessions, only 4 were listed in the Report, and 5 more have since been published by the U. S. Geological Survey. It is known that 8 additional sheets are in the process of preparation. Canada has produced only 3 sheets. Other large gaps in the project are eastern Siberia, China, and Australia. A large part of these areas has been mapped by war agencies on the scale of 1: 1,000,-

000, and these maps could be used to aid future work. At the present time it is possible that Australia might produce some sheets, but the situation in Russia and China will hold back mapping progress or prevent access to any sheets that may be produced.

Problem of Completion of the International Map

A need for completion of the International Map is more apparent today than ever before. The growth of international cooperation calls for the same cartographical area analysis as is common for political and economic problems within the boundaries of individual countries. In many cases the International Map would provide a possible background for such undertakings. Some of the other contributions which have been made by the map in the past are as:

1) A reference atlas. The map has served as an intermediate uniform cartographical reference in areal geographic research between large-scale topographic maps and small-scale atlas maps.

2) An aid to cartographical development. The map has helped to provide an opportunity for training cartographic personnel and to stimulate the development of national mapping programs.

3) A base map. The map has served as a base for work in the field, as well as in research studies which involve the recording and plotting of data. Under the latter should be included the use of the map in national planning for the comparison and correlation of various distributional and statistical features of regional economy.

4) A stimulus to other 1: 1,000,000 projects. Examples of this are the International Aeronautical Chart Series and the International Map of the Roman Empire.

5) A historical map. The various editions of any given 1: 1,000,000 sheet which have been published constitute a record of human occupancy in that area for the period covered.

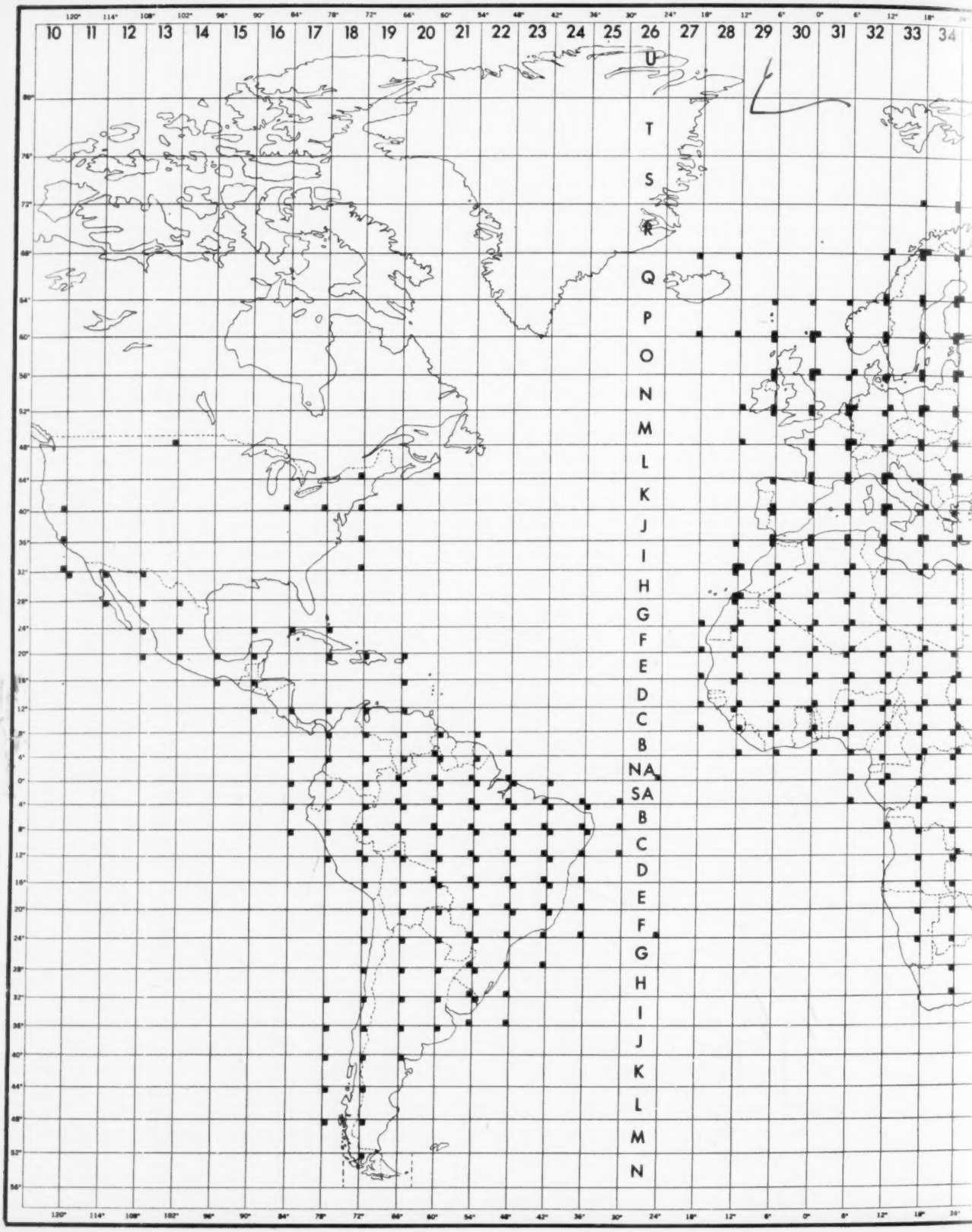
In view of these contributions and the increasing popularity and usefulness of the International Map, it would seem to be important that definite steps be taken to ensure the continuation of the project. The question arises as to whether the most effective way to prosecute the map is to continue its administration within the Central Bureau or to establish some other form of administration. With a knowledge of the origin of the International Map it is logical that the matter would be discussed at the meetings of the International Geographical Congress. Certain of these considerations were taken up at the Sixteenth International Geographical Congress held at Lisbon, Portugal, during April 1949. A new commission on the International Map of the World was set up under John K. Wright, of the United States. The Cartography Section of the International Geographical Union declared its support of this commission, which was to review the IMW, more especially with a view to its use as a base for the mapping of ecological and statistical data. A population map of the world on the scale of 1: 1,000,000 was included in the discussions.

The following possibilities are offered as suggestions for the solution of the problem of the future administration of the International Map of the World:

Supervision by the United Nations. The United Nations serving as administrator of the map has a logical appeal, and it would maintain the project on a strictly international basis. Perhaps the chief advantage of such a procedure would be the authority exercised by the UN as administrator. A more definite international agreement could be made among the adhering countries, and the centralizing force would be an international organization with the power to make binding decisions. Other advantages would be in the focusing of international attention upon the project, the active participation of most of the nations of the world, the cooperation made possible by frequent meetings, and the physical facilities provided by the UN organization. The major drawback would seem to be the dependence of the map upon the functioning of UN; any lack of progress, or dissension in the larger organization, would be reflected in the smaller project.

Merger with ICAO (International Civil Aviation Organization). This merger might provide economy and greater efficiency in the international administration of the map. Many of the operations and problems which the Central Bureau has handled for the IMW are duplicated in connection with the production of the International Aeronautical Charts by ICAO. This new organization has evidenced considerable strength, partly resulting from the world-wide interest in air navigation and travel. On the other hand, the merger might have distinct disadvantages. The objectives of the two maps are different, and such a combination might make each less effective or result in the dominance of one over the other. Furthermore, the sheetlines, style, and projection for the two maps are different, and the combined result might be a modification of the International Map.

Administration by the American Geographical Society. The work of this organization in producing the Map of Hispanic America leaves little doubt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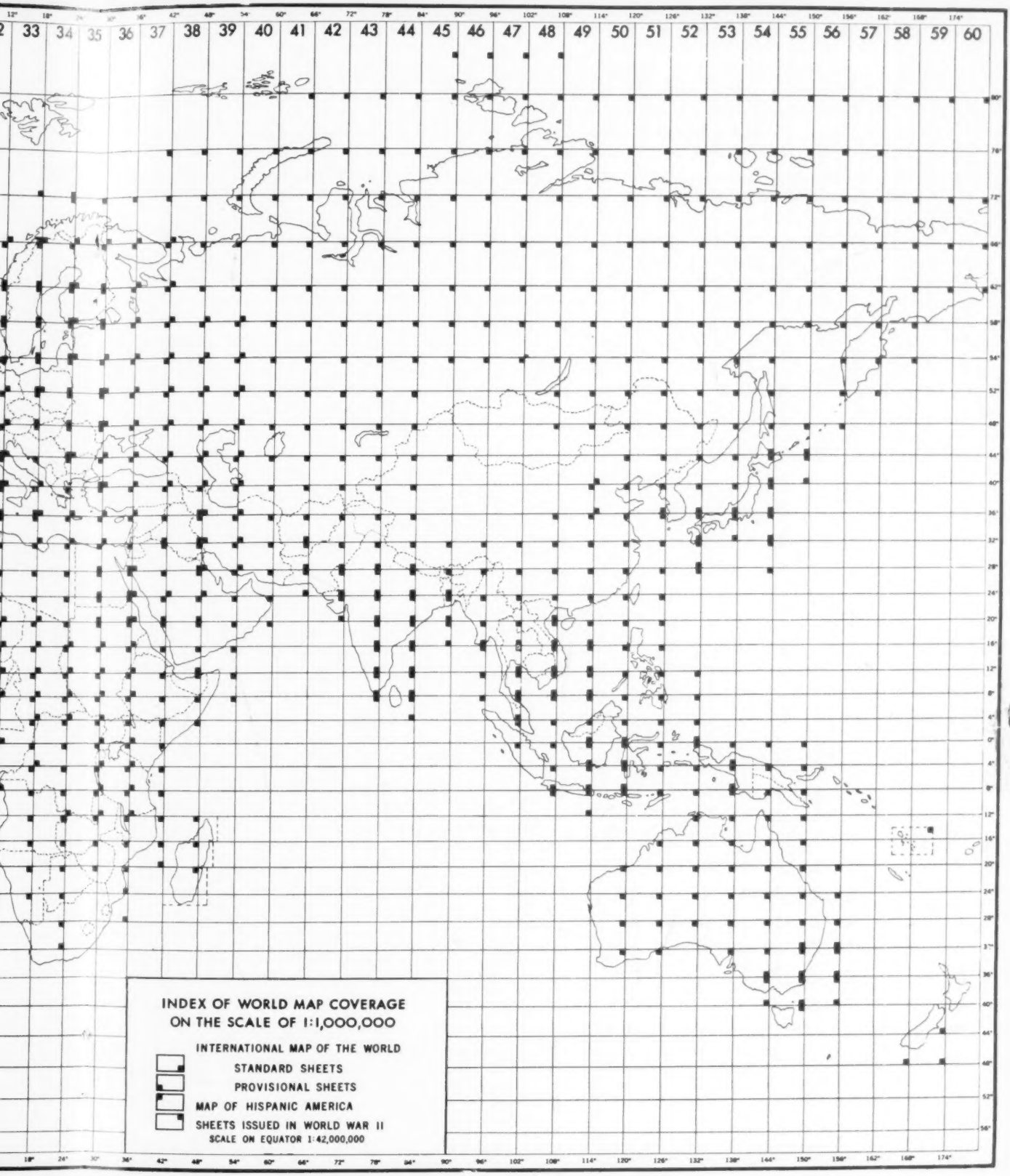
INDEX OF WORLD MAP COVERAGE
ON THE SCALE OF 1:1,000,000
INTERNATIONAL MAP OF THE WORLD
STANDARD SHEETS
PROVISIONAL SHEETS
MAP OF HISPANIC AMERICA
SHEETS ISSUED IN WORLD WAR II
SCALE ON EQUATOR 1:42,000,000

as to its ability in cartographic research. In addition, the society showed considerable administrative ability in dealing with different countries in Latin America. A major disadvantage of this procedure would be the close relationship of one country (United States) to an international project. Furthermore, as a private institution, the agency would lack the authority to enforce the resolutions and might encounter diplomatic problems. The heavy cost of such an undertaking would be beyond the means of any single private organization.

Continuation of the Central Bureau. The chief advantage in continuing the present Central Bureau would be in making use of the many years of experience which the bureau has had in administering the map. The bureau has never been entirely independent, however, but has been part of the British Ordnance Survey. Furthermore, the bureau has lacked administrative powers and to a large degree has been merely a service organization and clearinghouse. If the bureau continues as the operating agency for the IMW, some reorganization and strengthening of its position should be considered. The question is one of whether a more independent agency with a greater degree of administrative responsibility is necessary for the completion of the project.

Establishment of a new Central Bureau in the United States. The possibility of the United States as the home of a new Central Bureau should not be overlooked. Since the State Department is most concerned with matters of international status, perhaps the Cultural Relations Division of this body would be the logical place for such a coordinating bureau. The advantages of this choice would be the dissociation from the military, and the diplomatic and other facilities provided by the Division of Cultural Relations. As in the case of the British Bureau, however, the project would still have a strong tie to a single nation.

The previous possibilities have been evaluated purely from the standpoint of administrative potentialities. Of these it would seem that the United Nations would present opportunity for effective administration of the map as a cooperative international project. If, however, none of these choices for the completion of the project seems feasible or can be accomplished in the near future, the map is still important enough to be brought to completion. The great amount of wartime mapping makes possible the compilation of sheets for many areas which remain to be done under the IMW. Many of these were mapped on the scale of 1: 1,000,000. It is possible that the completion of the International Map can be accomplished by the cooperation of the two countries that did most of this mapping during World War II—Great Britain and the United States. Two possibilities seem evident:

Joint action of the governments of Great Britain and the United States. The Geographical Section of the British General Staff and the Army Map Service could cooperate in compiling the remaining sheets of the International Map. Additional information would be available from French, German, Italian, and Japanese agencies and maps.

Mutual cooperation of private geographical societies. The lead in this case would be taken by the Royal Geographical Society of Great Britain and the American Geographical Society of the United States. These organizations might be given some semiofficial support by their respective governments.

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The 1949 Eruption of Ngauruhoe*

PRESTON E. CLOUD, JR.†

The author, a member of the U. S. Geological Survey, while attending the Seventh Pacific Science Congress, witnessed the 1949 eruption of Ngauruhoe. The eruption was accompanied by the first active lava flow so far seen by persons of European extraction in New Zealand.

ABOUT twenty-five volcanic eruptions have been recorded in New Zealand in the century and a half since it has been inhabited by Europeans (beginning in 1839; see Thomson, 1926). On the other hand, during the same period, lava flows in this region were great rarities; the only historic records of such prior to February 1949 being reports of flows on the flanks of the volcano Ngauruhoe in 1869 and 1881 (Fig. 4 *D*). Moreover, although both these older flows are relatively fresh, and there is no good reason to doubt their authenticity, neither in 1869 nor in 1881 was moving lava actually witnessed.

The twenty-fifth recorded explosive eruption of a New Zealand volcano, and the sixteenth for Ngauruhoe (perhaps plus or minus one), occurred during the meetings of the Seventh Pacific Science Congress in February 1949, at the only time it would have been possible for the congress delegates to see it at close hand. The only active lava flow so far seen by persons of European extraction in New Zealand, and indisputably recorded in eruptive history, accompanied the 1949 eruption and was seen by at least four geologists of the U. S. Geological Survey and observed in actual, though greatly retarded, motion by three of us.

The volcano Ngauruhoe is one of a group of three major volcanic edifices comprising Tongariro National Park, in North Island, New Zealand (Figs. 3 *A*, *B*). This area is about 10 miles south of Lake Taupo, New Zealand's largest lake, and 80 miles south-southwest of the celebrated thermal district of Rotorua (Figs. 1, 2). It is located along the active fault zone that includes the Paeroa rift and the Whakatane fault, and this same general zone of faulting probably extends northward through the Bay of Plenty to the intermittently active volcano of White Island. It is also approximately on line with the great Alpine fault, which extends in a south-southwesterly direction the full length of the west side of South Island, and which has been interpreted by New Zealand geologists as a thrust fault with at least 9 miles displacement, as a transcurrent fault with about 300 miles lateral displacement, and as a fault that moved transcurrently in the early Cenozoic but by thrust movement later.

In more general terms, this same alignment may be projected northward for 1,000 miles beyond White Island through the Kermadec Islands and Tonga to Savaii, in the Samoa group (Fig. 1). (See also Hoffmeister *et al.*, 1929.) All three of these island-groups support active or dormant volcanoes and are bounded to the east by a great and seemingly continuous submarine trench that locally attains depths greater than 4,000 fathoms and is believed to denote a line of major downwarping of the earth's crust, a tectogene. Since 1774 at least forty-eight volcanic eruptions have been recorded along the New Zealand-Tonga line (Thomson, 1926, p. 369, and later sources), and the record is doubtless incomplete.

Although no active volcanoes are known in South Island, the zone in North Island along which Ngauruhoe lies is one of the great areas of volcanic and seismic activity of the earth. It is particularly noteworthy for its 200 cubic miles of rhyolitic welded tuffs, or "ignimbrites" (silicic, igneous rock), that mantle a region the size of Connecticut (about 5,000 square miles) northward from Lake

* Published by permission of the Director, U. S. Geological Survey.

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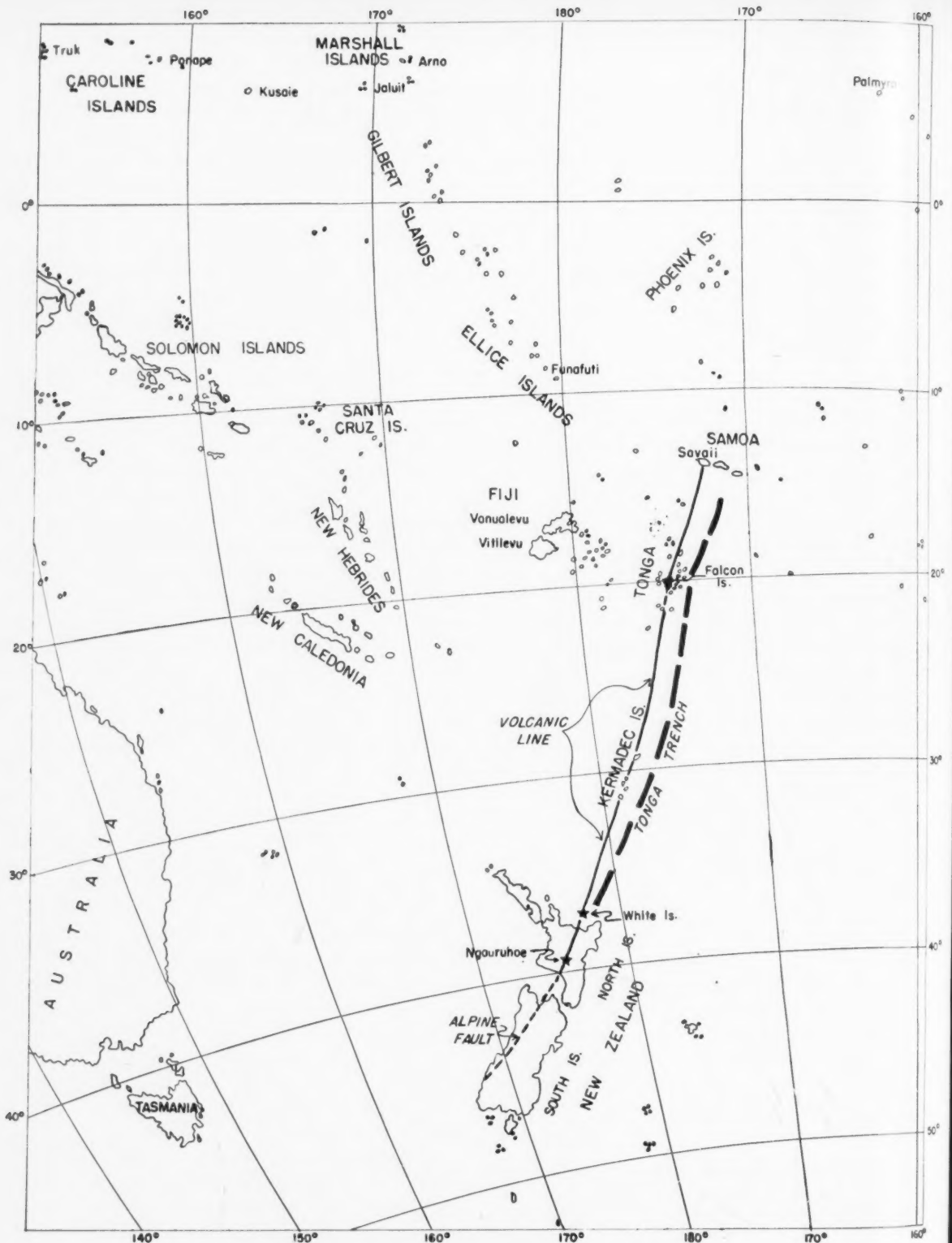
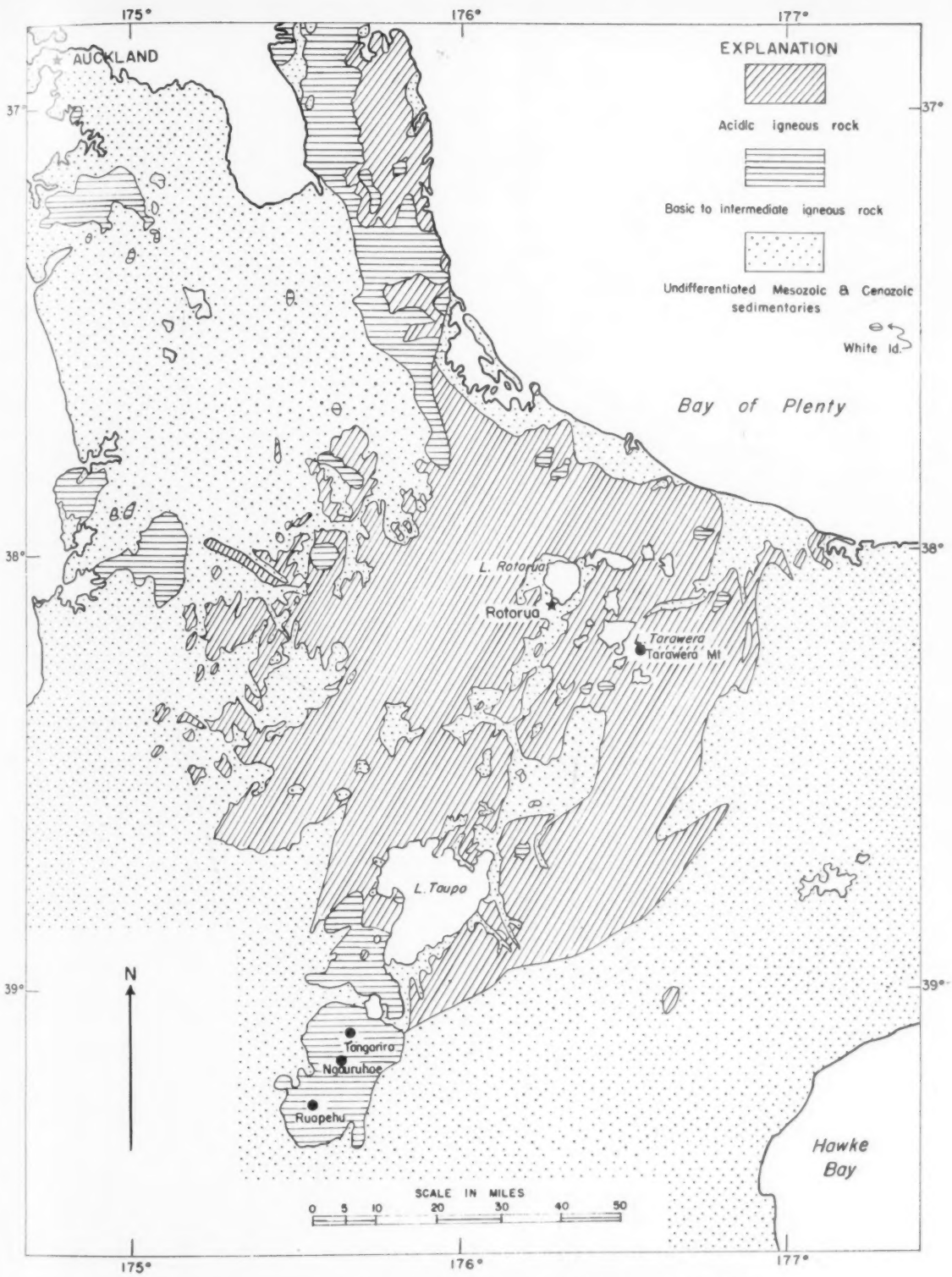


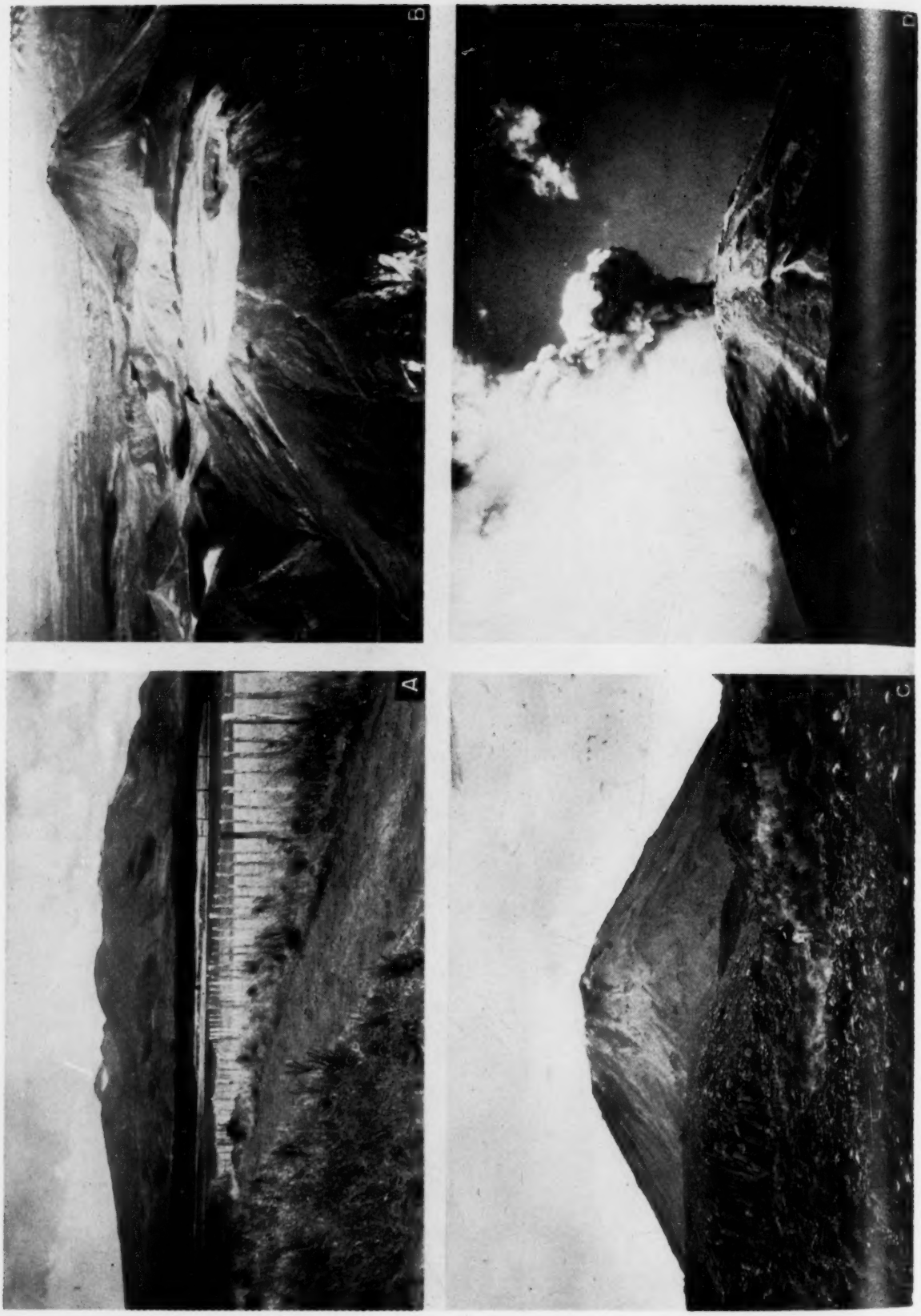
PORTION OF WESTERN SOUTH PACIFIC
SHOWING REGIONAL LOCATION OF NGAURUHOE

FIGURE 1.



PORTION OF NORTH ISLAND, NEW ZEALAND
SHOWING LOCATION OF NGAURUHOE
(adapted from Geological Map of New Zealand, New Zealand Geological Survey, 1947)

FIGURE 2.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Taupo (Fig. 2) and were erupted in prehistoric times as a succession of blasts of incandescently hot ash from longitudinal rifts or vents. The tremendous steam-blast eruption of Mount Tarawera, south of Rotorua, on June 10, 1886, was one of the great volcanic catastrophes of history—killing 153 people (147 Maoris and 6 Europeans) in a thinly populated area, showering two fifths of a cubic mile of volcanic ash and dust over an area of 6,200 square miles, and destroying or burying the renowned pink-and-white sinter terraces of Rotomahana (Leys, 1947). The products of this eruption, like much or most of the more recent eruptive materials along the Paeroa rift and its extension northward to Tonga and perhaps Samoa, are andesitic to basaltic ("basic" igneous rocks), rhyolite being represented by volumetrically less important pumice deposits and by fragments of disrupted and ejected older country rock. Essentially concordant activity between the Taupo-Rotorua district, White Island, and Tonga (Falcon Island) has been noted (Jaggar, 1945, pp. 23–29), although the significance that might be attached to such concordance is minimized by Thomson (1926, p. 370) and is probably incidental.

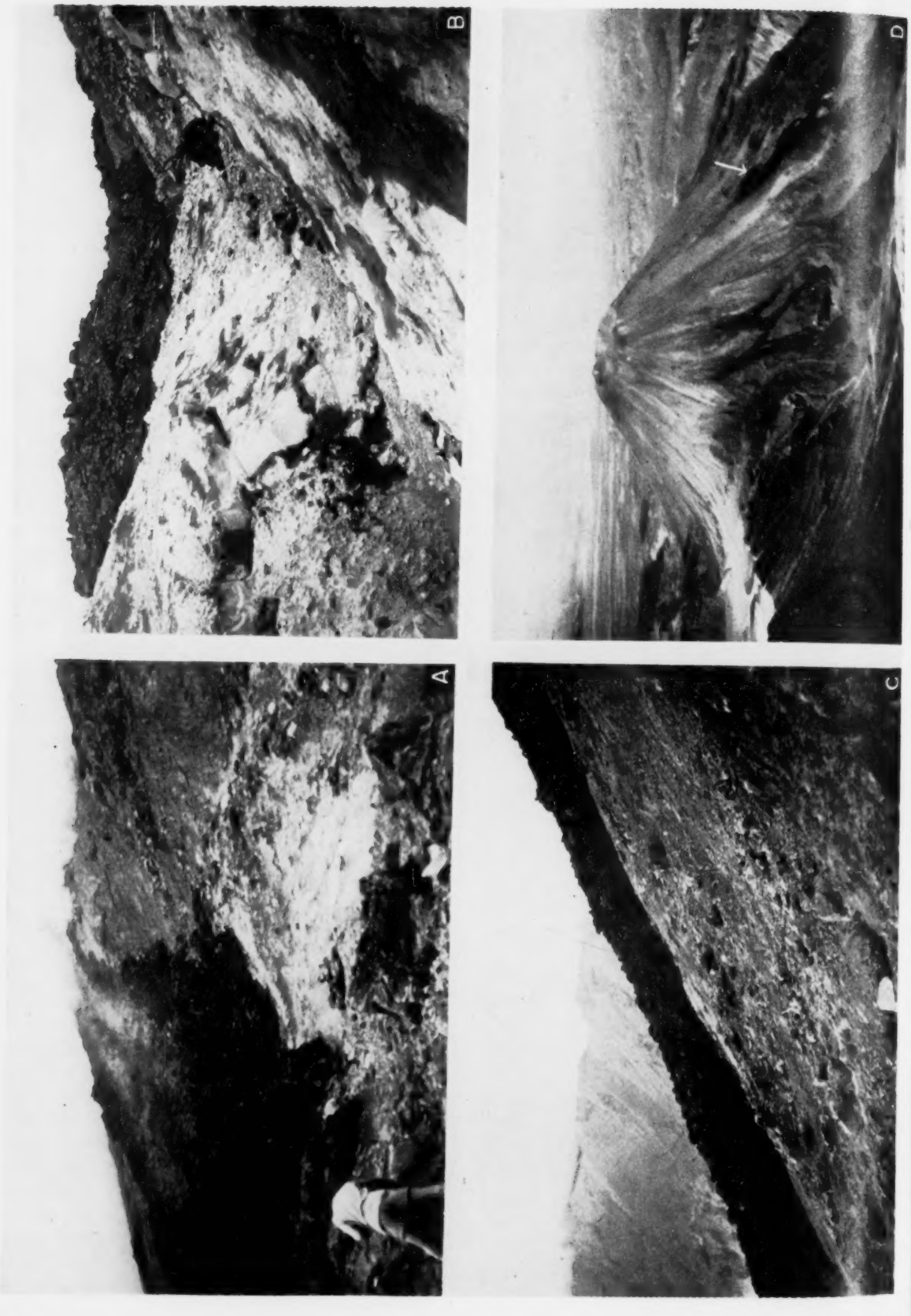
Ngauruhoe is, in any event, set upon one of the earth's major zones of rifting, seismic disturbance, and volcanic activity and has itself been the most persistently active volcanic center along this line within recorded history. Having considered its setting, we may now examine this particular volcano in more detail.

Ngauruhoe is a simple, symmetrical, ash and lava cone with upper slopes of about 30 degrees, an only slightly truncated top, and an intermittently active and continually changing inner crater about 100 feet deep (Figs. 3 *B–D*, 4 *D*, 6 *C–D*). Its structure is composite, consisting of andesitic and basaltic erupted debris of a wide size range, with intercalated and abutting flows, all combining to produce a strong and relatively stable volcanic edifice. It stands within a discontinuous "somma ring" (named from the ring of Monte Somma, around Vesuvius) that may represent an older, overtopped caldera. It rises 7,504 feet above sea level and about 3,500–4,000 feet above the surrounding rolling hills and moors of bunch grass, interrupted by occasional broader valleys that support forests of native beech. It overtops its northern neighbor Tongariro (6,458 feet), a dormant field of multiple volcanic activity of which it is considered to be a part (Fig. 3 *B*). To the south, Ruapehu (Figs. 5 *A–B*), another field of multiple volcanic activity, rises to 9,175 feet above sea level and is crowned with glaciers and persistent snow fields. Apart from a very brief though vigorous eruption of Ngauruhoe itself in early May of 1948, the most recent activity of these three volcanoes prior to 1949 was the wartime outbreak of Ruapehu (late 1944 to early 1946). At this time a tholoid mound rose within, and temporarily eradicated, the warm lake at the summit of Ruapehu (Fig. 5 *A*); intermittent blasts scattered ash in appreciable quantity within a radius of 10 miles of the mountain and to depths of several feet over the adjacent snow fields; and, following explosive disintegration of the tholoid, the eruption finally subsided to a solfataric phase (Cotton, 1946; Reed, 1945; Oliver, 1945; James Healy, informal communication).

The 1949 eruption of Ngauruhoe was of unusual interest in several respects. It was violent (of the grade of activity called "strombolian," Figs. 3 *D*, 5 *C*) but relatively short-lived. It apparently began with the blasting out of a plug of material from one of the crater's two intermittently active vents, followed by the issue of hot avalanches of the less gaseous Pelean type (Battey 1949, Pl. 70). It produced the first lava flow actually witnessed by persons of European extraction in New Zealand (Fig. 4 *A–D*), and it was observed at close hand by a relatively large number of geologists.

Samples of the lava flow and one of the still-warm erupted blocks (Fig. 5 *E*) were collected, and these were studied microscopically and by means of X-rays by petrologists of the U. S. Geological Survey. These specimens are dark-gray to black and range from fine-grained to conspicuously porphyritic, but all contain abundant phenocrysts. The feldspar in samples I collected is a plagioclase, on the calcic side of labradorite (anorthite 60–70%), although Battey (1949, p. 394) records zoned plagioclase ranging from 65% to only 39% anorthite. X-ray study of the pyroxene shows that it is mostly orthorhombic, consisting of pale brownish-green, weakly pleochroic hypersthene, with a few extra lines indicating the presence of a monoclinic pyroxene (definitely including diopside as determined by microscopic examination). Magnetite is very abundant in a vesicular groundmass that in-

FIG. 3. The setting of Ngauruhoe. *A*, Tongariro National Park from about 10 miles northeast of Ngauruhoe (arrow, vapor column to left). February 12, 1949. *B*, air view south across Tongariro (foreground), Ngauruhoe (right center), and Ruapehu (right distance). (Photograph by Donald E. White, February 28, 1949.) *C*, Ngauruhoe from the southwest, showing inner remnant of incomplete "somma ring" in foreground. Dark radial streaks on upper cone are gravitative concentrations of debris from 1949 eruption. February 13, 1949. *D*, strong blast of Ngauruhoe. (Photograph by Donald E. White, noon, February 26, 1949.)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cludes considerable basaltic glass. The calcic nature of the plagioclase makes these samples, at least, better classified as a basalt than as an andesite, although no olivine was seen in the thin sections examined (small amounts of olivine have been recorded, Battey, 1949, pp. 393, 394), and the previous erupted products have been reported by Cotton, Jaggar, Battey, and others to be hypersthene andesites. The lava contained a large number of inclusions of a coarse-grained andesitic rock that was doubtless derived from the underlying rocks through which the molten material made its way. A quantity of sulfur was seen as stalactitic drippings on the underside of one recently erupted block. The petrography of the lava flow is treated in more detail by Battey (1949, pp. 393, 394) who describes the rock as a hyalopilitic hypersthene-augite andesite. Battey also notes, however, that Grange, Williamson, and Hurst record basalt to occur west of Ngauruhoe.

Regardless of whether the rock is called an andesite or a basalt, it is genetically related to the andesitic products of volcanoes on the continental side of the theoretical "andesite line" that separates the Pacific Basin proper from "continental" portions of the present Pacific Ocean area. It seems to be a critical point that, among volcanic rocks on the "continental" side of the andesite line, flows comprise a relatively minor portion of total volume; whereas those in the Pacific Basin proper, such as the volcanic rocks of the Hawaiian Islands, are characterized by an abundance of flow rocks. For reasons not agreed upon, the "oceanic" magmas seem characteristically to be more basic and less viscous and explosive than the "continental" magmas. Flows of liquid lava are, thus, more characteristic effusive products of the "oceanic" province, whereas pyroclastic, or explosive, debris bulks large among volcanic successions of "continental" affinity.

According to the New Zealand *Herald* (Feb. 10, 1949, p. 6, account of R. H. Bates and T. P. Baker), the 1949 eruption of Ngauruhoe began at about 2:30 on the morning of February 9, following by about an hour a premonitory earth tremor that was accompanied by slight rumbling. It remained in relatively violent eruption until my visit on February 12 and 13, and was apparently only intermittently in strong eruption thereafter until the end of February. Strong blasts were witnessed by geologists of the New Zealand and U. S. Geological Surveys on February 26 (Figs. 3 *D*, 5 *C*) and 28; but when I flew over the volcano on March 5 the vent was sealed, the upper half of the flow was obliterated by eruptive debris, and only a wisp of vapor was issuing from the crater. Reports of further activity indicate strong steaming as late as May 18 and minor activity thereafter (Battey, p. 393), but the cycle of vigorous eruption apparently lasted not quite a month.

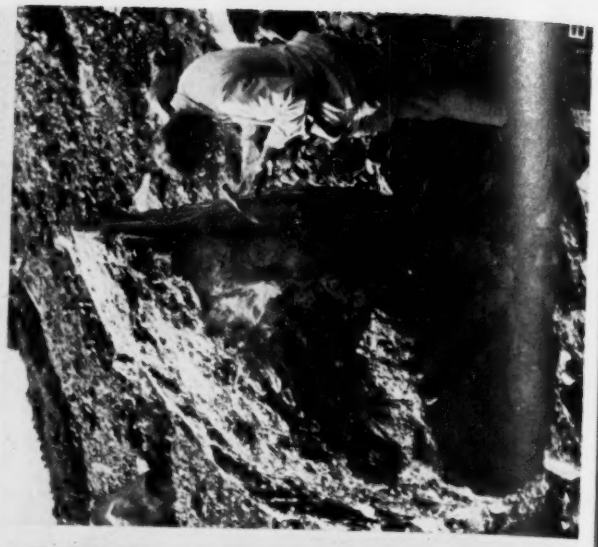
It appears that, about seven or eight hours after the eruption began, a series of blasts cleared out a vent toward the west side of the crater (Fig. 6 *C*) and deposited a mixture of detrital debris and molten material on the northwest side of the cone as a series of hot avalanches. Subsequently the crater of Ngauruhoe filled up with lava, which then flowed over the lowest part of the rim on the northwest side of the cone, at some time between 7:00 P.M. on February 9 and midnight of February 10 (Battey, p. 30). After the occurrence of hot avalanches had been inferred, and while the present report was in manuscript, it was learned that campers at Mangatepopo Hut (R. H. Bates and T. P. Baker), a little over 2 miles west of Ngauruhoe, had actually witnessed and photographed these hot avalanches (Battey, p. 389, Pl. 70). The flow that followed them moved for a little more than a mile down the northwest side of the cone, and was essentially stagnant about two days later, on the afternoon of February 12. Explosive activity lagged during the third day of the eruption (February 11), when climbers reported that eruptive activity was probably at an end and that no flow had occurred.

Meanwhile, those of us on a field excursion led by the New Zealand geologist James Healy had our first view of the eruption from high ground south of Lake Taupo 12 miles to the northeast of Ngauruhoe on the night of February 11. Activity had been renewed; red-hot blocks were being tossed to something like 2,000 feet above the cone, and an ash and vapor plume rose about a mile above it and drifted far off to the east in a strong westerly wind. A nearly continuous deep-throated rumbling accompanied the demonstration, but no quaking of the earth could be felt where we stood.

On the following day (February 12), while motoring toward Ngauruhoe along its west side, we noticed what appeared to be quite clearly a very recent flow on the northwest side of the cone. Thus, on reaching the hostel Chateau Tongariro

FIG. 4. Lava flows of Ngauruhoe. *A*, view up the 1949 flow from near its lower end, showing median and lateral ridges. Part of preceding avalanche track at right. February 12, 1949. *B*, one of terminal lobes of 1949 flow. Path of hot avalanche in foreground. February 12, 1949. *C*, side view of 1949 flow from just outside the avalanche track. February 12, 1949. *D*, air view from north showing 1949 flow at right (arrow) and, at center, older flows of uncertain date of issue (1869, 1881). The upper ends of all flows are covered by erupted debris, and their lower ends are lobate. (Photograph by Donald E. White, February 28, 1949.)

B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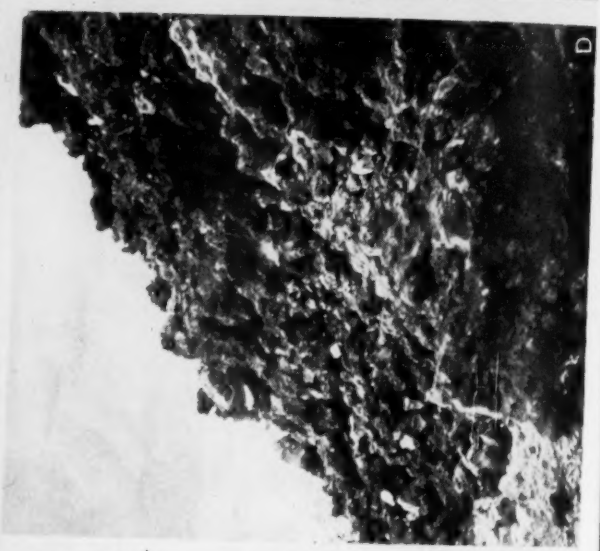
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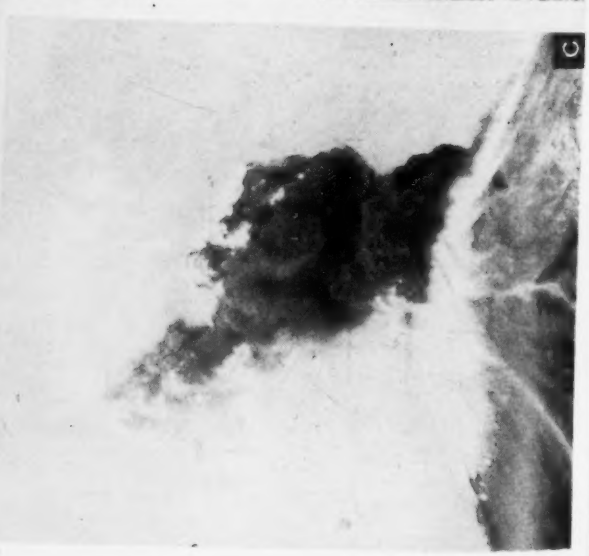

C

at noon, we immediately made preparations to hike to the flow, 7 miles distant.

Healy and I climbed to a point about halfway up the cone of Ngauruhoe, at the flow's edge and at the bottom side of a heavy cloud cover that concealed the peak. At this place blocks of still-hot hypersthene basalt (and probably andesite), many larger than 6 feet in diameter and some with dimensions up to 15 feet, lay all about (Fig. 5 *E*). The flow (Figs. 4 *A–D*, 5 *D*) extended about a mile down the northwest flank of the cone and was about 100 yards wide in its upper part and perhaps 150 yards wide at places toward its lower terminus. It displayed leveelike sides that rose to an average of about 15 feet above the general slope of the cone, was ridged in the center, and had longitudinal gutters between the central ridge and the lateral levees (Fig. 4 *A*), this appearance, presumably, resulting from the combined effects of continued downflow of molten lava beneath a slower moving crusted surface and mechanical piling up of solid fragments at center and sides. The lava was of the blocky, or aa, type (Fig. 5, *D*), in a late and sluggish stage of movement. Movement observed was of a piecemeal, or "tractor," type, the upper portions of the flow falling free at the front and very slowly advancing over the basal part, much in the manner of the more sluggish movement of flows at the Mexican volcano Parícutin; but its gross characteristics show clearly that early movement was much faster and more fluidal. Only a little glowing material was seen, though pieces broken from the margins were too hot to be held in the unprotected hand. At its front the flow had divided into several lobes (Fig. 4, *D*) where it flowed around topographic protuberances at the basal flank of the cone.

Unfortunately, it was too late in the evening to study the flow in detail. On the way down the lower half of the mountain, however, it was noticed immediately that the adjacent slopes had a scoured appearance and that dust and rubble in the low places and in advance of the flow were relatively loose and uncompacted and felt hot even through heavy boots (Figs. 4 *A–B*). The "scouring" had also smoothed off rough surfaces and stripped away much of the soil and vegetation cover.

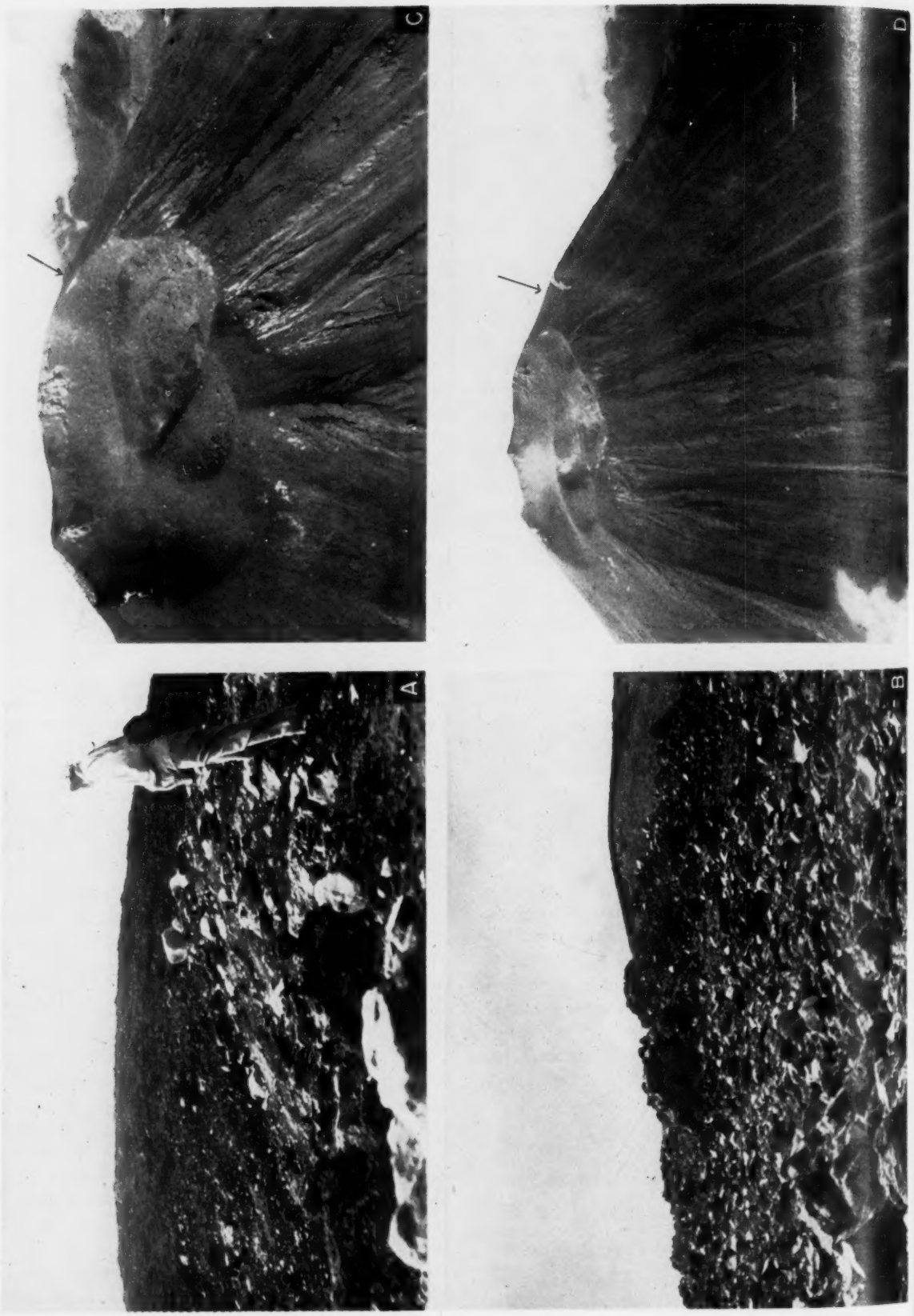
FIG. 5. Ngauruhoe and Ruapehu. *A*, air view of Ruapehu from southeast, showing crater lake and veneer of ash on snow fields. March 5, 1949. *B*, Ruapehu, viewed south-southwest from Ngauruhoe. February 13, 1949. *C*, major blast of Ngauruhoe. (Photograph by Donald E. White, 11:12 A.M., February 26, 1949.) *D*, view northwest along side of Ngauruhoe's 1949 flow, showing blocky nature of marginal lava. February 12, 1949. *E*, James Healy examines one of larger blocks of Ngauruhoe's 1949 eruption, near edge of flow. February 12, 1949.

In the meantime, D. E. White and others had taken a different route to the base of the flow, and they paid particular attention to the hot debris noted above. That evening White made the logical suggestion that it probably represented the deposit of a hot avalanche that had swept down the northwest side of the cone, giving it a scoured appearance in places, and accumulating mostly at the base of the cone. The flow seemingly followed in the path of this avalanche, burying its track in large part and riding out over the avalanche debris at the lower end.

On the following day (February 13) it was necessary for Healy to go on to Wellington with his field excursion, but White, Charles Johnson, and I remained to investigate further whether the flow had actually been extruded over the lip of the crater, and whether it had been preceded by a hot avalanche or avalanches. We started at 5:30 A.M., White and a companion returning to examine the avalanche deposits and Johnson and I, with five New Zealand companions, setting out to climb to the crater. As a result we were able to verify that the flow had issued by overflow from the crater itself, and White to conclude that a series of hot avalanches had occurred and that at least one (and probably all) of them had preceded the flow, a conclusion now established by photographic evidence (Battey, Pl. 70).

White recognized several distinct hot avalanche deposits, the largest of which, in advance of the flow, was a few acres in extent and had a maximum thickness of perhaps 25–40 feet. The borders were sharp, and the avalanches had apparently moved down the slope along the main surface drainage lines. The surface of the largest deposit was soft and hot, and marked by many small fumaroles. The temperature of one of these furmaroles was 293° F. on February 13, but had cooled to 222° F. by February 26 (both readings by D. E. White). Three months later, in mid-May "at an air temperature of 54° F., a thermometer laid on the sand [of one of the avalanche deposits] registered 92° F., while at a depth of three inches the temperature was 122° F." (Battey, p. 390). The largest boulder in the heterogeneous mixture of avalanche debris was found by White to be 26 by 18 by 12 feet. On a later visit, following washing by rains, he estimated that about 75 per cent of the coarse material had come directly from the crater, and about 25 per cent had been picked up from the slopes, the latter being characterized by weathered andesite containing the prominent feldspars that are distinctive of the prehistoric eruptive debris.

The ascent of Ngauruhoe began in heavy fog;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it was not until 10:00 A.M. that the mist and clouds began to lift and that the top of the cone could be glimpsed from time to time. Major detonations occurred at intervals of two minutes to two hours; intervening activity consisted of a persistent emission of gases, punctuated by lesser explosions during which blocks, lava, and subordinate ash were tossed far above the wide crater and fell back into or near it. Accompanying the milder activity was a loud and rhythmical chugging or panting noise that sounded like a locomotive pulling a heavy load on a steep grade—reminiscent of the sound effects that accompany the lava fountaining at Parícutin, and indicative of large-scale emission of gases.

At 11:25 A.M., when we were about 600 feet (vertically) below the lip of the crater, a major blast occurred, followed two minutes later by another (e.g., Fig. 5 *C*). Large blocks were thrown an estimated 2,000 feet or more above the crater, and debris showered out of the air and avalanched down the slope of the cone around the large block in whose lee shelter had been found. Molten lava was flung out of the crater, along with solid blocks and ash, separating in mid-air and depositing itself as crustlike patches that molded to the surface where they landed and froze. A haze of dust resulted from the settling of the finer erupted debris and the dust raised by the impact of larger blocks on the surface of the cone. Shelter beyond this point was insufficient for the entire group, so after lunch only three of us continued, stopping periodically to observe the lesser intervening activity and to rest. At 1:45 P.M., when we were only about 100 feet vertically below the lip of the crater and perhaps 400–500 feet away from it (Fig. 6 *A–D*), we were again driven to shelter by another giant blast. When the dust settled, it was observed that there was no further cover between us and the crater—only an open surface of black and smoking recently erupted debris, into which it would have been too hot to dig and which offered no large blocks to crouch behind (Fig. 6 *A*).

In view of the erratic timing of the individual detonations, it was decided not to advance farther. As it developed, the next blast did not occur until about forty-five minutes later and the climb could easily have been completed. Our wish to observe the interior of the crater had not been satisfied, but we had been able to see the top of the flow and to confirm that it must have resulted from overflow of a lava-filled crater.

FIG. 6. The top of Ngauruhoe. *A*, high point of ascent; Charles Johnson looking toward rim of temporarily quiescent crater on horizon. February 13, 1949. *B*, upper slopes of cone at southwestern side, surmounted by gas-cloud phase of mild activity. February 13, 1949. *C*, and *D*, air views of crater and cone from northwest and west, taken on March 5, 1949. Explosive phase of the 1949 eruption completed and top half of flow buried under erupted debris. Note lava plug in bottom of crater and small gas cloud at east side of main subcrater. Arrow marks high point of ascent.

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It is not the *possession* of knowledge, of irrefutable truths, that constitutes the man of science, but the disinterested, incessant *search* for truth.

—Karl Popper, *Logik der Forschung* (1935)

Karl Marx and the Social Scientists

MARGARET T. HODGEN

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WITH a loyalty cum oath epidemic threatening to sweep the universities and colleges of the country, three facts stand out clearly. The American public is losing confidence in the professors; the professors, in turn, and especially those in the social sciences, are being placed in an untenable and unjustifiable position; and an academic crisis impends, involving not only academic freedom and academic responsibility, but the maintenance in our institutions of learning of adequate analysis of Marxian doctrine or Communist social theory.

The reason for this state of affairs, so inimical to the proper conduct of scientific inquiry and instruction, is not far to seek. It is fear. It is feared by the public that the professors, and notably those concerned with the teaching of social theory, are easily fooled, and, if not actually alienated from Western democratic ideas, at least politically naïve. It is feared that their youthful students are gullible, or incapable of adult, independent judgment in the presence of their bemused professors. It is feared, above all else, that Communism, or the theoretical foundation upon which it rests, may seem to be too attractive, too persuasive—or even seem too true—to be permitted objective analysis or to be given an intellectual airing.

On the campuses of many of our universities, the professor is also becoming afraid, and not without cause. Though he may recognize, as a competent inquirer in the fields of economics, politics, anthropology, and sociology, that the methodological ideas upon which Communism rests are unsound and easily refuted—and though he may know, from long experience, that classroom criticism has been materially instrumental in arresting the spread of extramural Communist propaganda at the gates of the campus—he now finds himself a marked man. For, if he continues classroom analysis, if he subjects Marxism to scientific scrutiny—if, perchance, he even allows Communist social theory to be raised for debate—he may jeopardize his job, endanger the security of his family, and court the loss of his good name. Although half the world's population has embraced Marxism, dialectical materialism, the theory of the class struggle, and other tenets of Communist doctrine, fear bids fair to check their exposition and criticism for the benefit of American students. As fear has enjoined silence in the universities of Soviet Russia concerning the too-attractive, too-persuasive ideas underlying democratic societies, and has brought disrepute to Russian scholarship, so now in American institutions of higher learning fear is about to impose silence upon the competent criticism of Soviet doctrine, and to undermine the professional integrity of the American professorate.

The gravity of this situation, both for the nation and the universities of the country, cannot be exaggerated. The formulation of effective foreign policy depends upon knowledge rather than ignorance, and upon the maintenance of a constant flow of well-informed and well-trained men into posts of political power and responsibility. On the other hand, knowledge of other peoples, and of the political and economic ideas that inform and motivate their policies, is not a natural and automatic endowment of those who chance to assume public office. If they have it, it has usually been communicated to them along the channels of their formal and higher education and during the course of instruction in the social sciences. The responsibility

of the educator in the social sciences is thus twofold. Each generation of students must be given access to every avenue of information concerning the cultures of other peoples, and the metaphysical, economic, and sociological presuppositions that govern their behavior. But, at the same time, especially at this moment in history and with public misapprehension of the critical function of scholarship being what it is, the public must be reassured. To regain public confidence, the professors must inform the public concerning the content of their instruction on these moot subjects.

What, then, is the point of view of professors in the social sciences with reference to the scientific pretensions of Marxism? What are students being told concerning the validity of Communist social theory by professors who are academically "free"? How in classrooms, where the professors have not been already muzzled, is the uncritical propaganda of extramural party-liners and fellow-travelers being answered?

II

There are many facets to Marxian social theory, but underlying all else as a logical and methodological scaffolding is the Marxian philosophy of history —the materialistic conception of history, the economic interpretation of history, or economic determinism.

Generally speaking, and for most people, the word "history" contains few difficulties. To the man in the street it is a boring subject taught in the schools. To our own unhistorical generation, it refers to the record of the deeds and misdeeds of men so long dead that it is considered a waste of time to consult it. To the initiated, however—to the historian or the scientist or the philosopher—history poses many embarrassments and perplexities. For the word "history," taken to mean the past of humankind (a narrow definition at best), may refer to the documentation of the totality of the human past, in both its archaeological and written form; or it may refer to what historians do with these documents as they write their histories. The word history may point to that immense but in part unused documentary store of information concerning the lives of numberless human beings and their multitudinous activities from the creation of the world to the present; or it may point to that vast array of historical narratives written by members of the guild of historians by virtue of their access to this relatively unexhausted store of documentary information. In any case, whether already written or as yet unwritten, every individual who has ever lived in the world has had a history. Every hamlet, every village, every town, every county, state, and nation has had a history. As Frederick J. Teggart, that great student of historiography, has said, "History is not unitary but pluralistic." Though the word "in its widest sense means all that has happened in the past, and more particularly, all that has happened to the human race," as individuals or as members of organized groups, what the historian is given when he inspects the human past "is not one history but many." As many as there have been human inhabitants of the earth: as many as there have been small or large agglomerations of mankind.

It is here, as the social scientist engaged in evaluating Marxism will point out to his students, that the philosopher of history enters the picture. He enters to overcome pluralistic chaos, to form a synthesis, or to come at the "meaning" and "significance" of the "whole" of human history. In the presence of the "infinite particularity and multiplicity" of recorded events, as they have taken place in every land and every clime, the philosopher attempts to grasp the totality of human experience under one formula, within one single chronological framework, or in abstraction from the particular histories of particular individuals and peoples. He endeavors, with the aid of the conceptual apparatus of philosophy, to select from the chaff of countless multitudes of events, affecting countless human beings throughout the entire span of man's residence upon the planet, those few episodes that appear to have been common everywhere and at all times. He seeks to arrive, at least along the avenue of speculation, at a theory of the historical process.

There have been many philosophies of history, the student will be told. At one time during the career of Western man, all men subscribed to a Christian synthesis formulated in the fourth century by St. Augustine. Not only did St. Augustine assume that the historical process was everywhere the same, not only were all peoples everywhere involved in the working out of God's plan for man, not only did the working out of this plan presuppose a conflict between good and evil, or between good men and bad, but the process was prophetic, apocalyptic, and Messianic. It terminated for the good and the saved in the nonterrestrial, eternal, and heavenly City of God.

At a later time in the seventeenth and eighteenth centuries, or after the secular triumphs of science, this view of history, as the story of God's way with man, was succeeded by another. In the so-called quarrel between the ancients and moderns, extensive comparisons were made between the achieve-

ments of classical man and modern man, and the Idea of Providence was supplanted by the Idea of Progress. Progressive historical change was not only envisaged as natural and necessary, but being accepted as necessary, was also taken as an assurance to mankind of continued and increasing well-being in the terrestrial world. The rapid advances of science created an optimistic belief in the perfectability of mankind and society.

The Idea of Progress, in short, became a theory which henceforth, for three hundred years, served as an organizing principle in the study and contemplation of man. Philosophies of history built upon this principle of inquiry followed one another as fast as the printing presses could turn them out. To Immanuel Kant, the human race appeared to be "continually advancing in civilization and culture" and continually making progress in relation "to the moral end of its existence." To the Marquis de Condorcet—philosopher, mathematician, and prophet of the liberal revolution—nature had assigned "no limit to the perfecting of the human faculties," and the ultimate perfectibility of mankind, foreshadowed by the history of his past accomplishments, was regarded as "truly indefinite." To Auguste Comte, the father of sociology, history was epitomized in the progressive advancement of knowledge; to Hegel, Marx's teacher, in the progressive realization of freedom. Indeed, as late as 1942, Julian Huxley in his *Evolution: The Modern Synthesis,* closed his argument with the question, "Is evolutionary progress a scientific fact?"

The Communist interpretation of history, as students will be informed, or as it is expounded in *Capital: a Critique of Political Economy,* and in other Marxian writings, is also a philosophy of history. To Marx, human history is not a number of parallel histories, but one single history. It is a restatement of the traditional generalization that "all history is the progressive modification of human nature;" of the traditional and ancient and religious belief in the conflict between good and evil, as manifested in the struggle of good and bad social classes; of the traditional and ancient and Hebrew faith in the final consummation of human historical experience in an apocalyptic, Messianic, universal, and perfect Communist condition. "Since 1917," says Arnold Toynbee, "we seem to see Marxism turning, before our eyes, into an emotional and intellectual substitute for Orthodox Christianity, with Marx for its Moses and Lenin for its Messiah and their collected works for the Scriptures" of this new Russian church militant. To this philosophical conception of the past, sanctified by faith and antiquity, Marx adds only one new element, derived from his preoccupation with the phenomena of industrialization. This is the dictum that progressive change in the economic activities of man—variously phrased as "the mode of production," or "the material forces of production," or "the form of production," et cetera—precedes progressive change in all other realms of human activity. Thus it is claimed that the origin of all other social institutions is accounted for, including religion, the political state, the family, philosophy, science, morality, and the arts.

As the architect of this ambitious philosophical and historical edifice, Marx felt compelled, however, to indicate the sequence of epochs or stages initiated by historically prior changes in "the tools of production" or "the mode of production." These steps in the working out of his law of human history from its earliest beginnings to its inexorable destiny are variously statable as three or five. Historical progress opens for Marx with a "prehistoric" tribal or classless type of society in which the tools of production are owned in common by all members of the tribe. This "prehistoric," first, original, and earliest communal stage or epoch is succeeded universally among all human groups by a second, or "historic," period, characterized broadly as the stage of private ownership. In the second epoch, as is well known, society purportedly is subjected to opposing class divisions, and thereafter traverses three substages in which the economy is described, first as "antique;" second, as "feudal;" and third, as capitalistic. Meanwhile, the masses of the human population become, first, slaves, then serfs, and then propertyless wage slaves. Finally, according to the apocalyptic, Messianic dénouement, the "historic" period embodying this succession of substages is succeeded by the predicted future condition which, restoring mankind to the Golden Age of communal ownership and a classless social organization, is ushered in by the "the dictatorship of the proletariat." Marx holds, says Joseph Stalin in his pamphlet on *Dialectical and Historical Materialism* in the Little Lenin Library, that the process of historical development "should be understood . . . as an onward and upward movement . . . as a development from the simple to the complex, from lower to higher. . . . Hence, social life, the history of society, ceases to be an agglomeration of 'accidents,' and becomes the history of society according to regular laws, and the study of the history of society becomes a science."

As shown by this quotation from Stalin, it was characteristic of Marx, as it is also characteristic of many other philosophers of history, not only to

refer to this succession of stages as the natural and historical order of the appearance of new social forms, but to refer to his method as scientific, and his findings as comprising a science of history. In the preface to *Capital,* Marx speaks with approval of a reviewer who states that "the one thing that is of moment to Marx is to find the law of the phenomena with whose investigation he is concerned. . . . Consequently, Marx only troubles himself about one thing: to show by rigid scientific investigation, the necessity of successive determinate orders of social conditions, and to establish, as impartially as possible, the facts that serve him for fundamental starting points." For this inquiry, "the one thing of moment is" that the facts "be investigated as accurately as possible." Elsewhere in the same preface, Marx states that the ultimate aim of the *Capital* is to "lay bare the economic law of motion in modern society," or those tendencies "working with iron necessity toward inevitable results."

As a matter of fact, Karl Marx, as a student of social or economic change and human history, has often been compared by Communists to Charles Darwin as a student of biological change and natural history. Indeed, Marx often spoke of the "natural history" of society and regarded the *Origin of Species* "as a basis in natural science for the class struggle in history," though it was developed, he says, "in the crude English style." It should be noted, however, that Darwin worked at a great disadvantage. Although his problem was a historical one—the history, in a sense, of genus *Homo,* in terms of an antecedent series of ascending biological forms—and although he recognized it to be a historical problem, he had access to little or no historical documentation, apart from a few fossilized fragments of ancient fauna and flora collected by the curators of geological museums. "I look upon the natural geological record," he said, with becoming scientific frankness, "as a history of the world imperfectly kept, and written in a changing dialect; of this history we possess the last volume alone, relating to only two or three countries. Of this volume, only here and there a short chapter has been preserved; and of each page, only here and there a few lines." This careful scientist therefore acknowledged "a crowd of difficulties" or many lacunae in the evidence of his theory—so many that he confessed himself to be "in some degree staggered." Marx, on the other hand, in contrast with Darwin, was very favorably placed. As a scientist and historian he was richly provided with historical documentation. Though the written or printed record of man's past can nowhere be considered complete, it covers in illimitable detail at least four thousand years, and refers to the historical experiences of men living not only in all the countries of Europe, but in the ancient Mediterranean East, in India, in China, in the New World, and in many other, smaller regions. And to this must also be added the archaeological record which carries the inquirer back far beyond four thousand years in many of these areas.

III

How, then, as a scientist, and by "rigid scientific analysis," did Marx employ this wealth of documentation? To what extent, as a scientist, did he make use of it to verify those tendencies "working with iron necessity toward inevitable results"? In how many particular histories of particular peoples did he find the sequence of stages which he describes as universal? To what extent, and in conformity with the ethics of science, did he report *negative* instances or *acknowledge difficulties* in verifying his philosophy of history? The historical order Marx offers to his readers is either supported by historical documentation, or it is not. No matter how subtle may be the philosophical convolutions in which the materialistic interpretation is wrapped, the test of its scientific and historical accuracy is its conformity with what actually happened in history. Before the public attempts to silence the critical efforts of the social scientist and compels him to retreat in dismay from the candid analysis of the Communist interpretation of history, it should know how he handles these questions before his classes.

The answers, of course, are brief and blunt. As a historian, students will be told, Marx was naïve; as a scientist, he was elusive. There are more than eight hundred pages in the English edition of the *Capital* issued by the Modern Library and approximately 27,000 lines of print. Historical references appear on only about fifty-five pages; and since nearly all of them are given only a few lines each, the number of pages devoted to the historical documentation of this theory of history is something like twenty-five or thirty out of eight hundred. Compare this with Arnold Toynbee's *Study of History* which, as another philosophy of history, is given thirteen volumes of documentation. Consider also Toynbee's judgment that there have been twenty-one civilized societies during the course of man's life on the planet, and more than 650 non-historical or primitive groups and tribes. Whether or not Toynbee's judgment on this point be acceptable to all historians and anthropologists, Marx mentions, and merely mentions, only a small frac-

tion of each. Though Marx definitely states that "viewed apart from real history . . . abstractions have in themselves no value whatsoever" and "serve only to facilitate the arrangement of historical material," he himself, unlike Toynbee, feels no obligation to assemble historical material in any degree. He relies almost wholly upon his own a priori judgments and his own view of logic.

From the standpoint of scientific and historical method, or the way he introduces his very infrequent references to dated evidence, Marx's procedure is also subject to adverse comment. In the *Capital,* as is well known, the author addresses himself to an analysis of the existing capitalist system. His purpose is not only to disclose its defects, the signs of its imminent dissolution, and its intimations of the inevitable emergence of a perfected Communist economy, but also to trace its historical antecedents in the form of a succession of preceding economies. For this, instead of a detailed investigation of all economies and their antecedents, on all areas inhabited by historical man, with induction from a comparison of their histories, Marx chose rather the device of illustration and, moreover, illustration from one little island off the coast of the Eurasian land mass with its teeming peoples and diverse economies. England, he said in defense of this patently indefensible historical short-cut, is classic ground—the first area upon which capitalism had yet appeared. Despite the presumed uniqueness of this economic phenomenon not only in England but in the histories of other peoples, Marx adopted the helpful but unsupported assumption that being industrially more developed—in the van of progress, so to speak—the English economy showed to all the less progressive economies "the image of their own future." A "strange optical illusion," he was to say later of John Stuart Mill, whom he charged with advancing a similar argument, "to see everywhere a state of things which as yet exist only exceptionally on our earth."

At the same time, Marx's consideration of English economic history was not inclusive of its whole recorded span. The stages preceding the capitalist stage were not documented in even this one history of one people. Though the English historical experience, terminating with a capitalist economy, was taken as a prototype to be reiterated universally on all other areas inhabited by man, he dealt with at most four centuries of the English record. He began with the sixteenth century and ended with the nineteenth. All that went before in this ancient land, which had been set down in its annals by chronicler after chronicler and historian after historian, was lumped together in scattered and casual references to an "antique" economy, or to the practices of a prefeudal or feudal economy. Nor were the precapitalistic stages delineated in the history of any other people.

More than this, historical illustration rather than arduous investigation is the procedure followed throughout the *Capital*: or, more often than not, personal historical judgments are made in the form of simple declarative sentences without illustrative or any other form of evidence.

"In *all* states of society," says Marx inclusively, "the labor-time that it costs to produce the means of subsistence must necessarily be an object of interest to *mankind,* though not of equal interest in different stages of development." To support this typically sweeping statement concerning *all* states of society and *mankind* at large in *differing historical* stages of development, Marx cites von Maurer on *one* people, the ancient Germans, and *one* of their customs—the measurement of land according to the amount of grain that could be harvested in one day. This and nothing more.

Again, "the conversion of fractional work into the life-calling of one man corresponds," says Marx in another place, "to the tendency shown by *earlier* societies to make trades hereditary; either to petrify them into castes or . . . to ossify them into guilds. Castes and guilds *arise* from the action of the same natural law that regulates the differentiation of plants and animals into species and varieties, except that, when a certain *degree* of development has been *reached,* the heredity of castes and the exclusiveness of guilds are ordained as a law of society." For this sweeping historical statement concerning the heredity of occupations and the origin of castes in many or all societies, the only support is a footnote quotation from Diodorus Siculus, writing in the first century after Christ, on the artificers of Egypt at that date; and a textual reference to the weavers of Dakka and Coromandel in India from a source written eighteen hundred years later, in 1832. This and nothing more.

Again says Marx, with characteristic disregard of the need for scientific and historical evidence, "Cooperation based upon the division of labor, in other words, manufacture, *commences* as a spontaneous formation. *So soon* as it attains some consistence and extension, it becomes the recognized, methodical and systematic form of capitalist production. *History shows* how the division of labor . . . acquires the best adapted form *at first* by experience . . . and then, . . . strives to hold fast that form . . . and here and there succeeds in

keeping *it for centuries.*" But history does *not* show, or at least Marx does not show that history shows. There is no evidence adduced for any of the several historical elements of these declarative sentences.

In the *Capital* we are also met by many pronouncements such as this: "Nomad races are the *first* to develop the money form because all of their worldly goods consists of movable objects and therefore alienable; and because their mode of life, by continually bringing them into contact with foreign communities, solicits the exchange of goods." Marx feels no obligation to support the asserted historical priority of the nomadic peoples in the invention of money, nor does he consider it necessary to assemble proof of the soundness of his historical explanation for their priority.

In response to these criticisms, of course, defenders of the Communist interpretation of history will rise to their feet to say that Marx was not engaged in assembling grubby, finical historical detail; that, as a philosopher of the progressive or perfectibilitarian persuasion, his aim was to state generally, theoretically, ideally, and hypothetically the historical transition of economic society through a series of normal, natural, and progressive stages from primitive communalism to modern Communism. This is true in the sense meant. But readers who are expected to take the series of stages as scientifically true have a right to call for scientific verification.

IV

In addition, every conjectural history of this type must start somewhere in the human past. However many stages a stage-series may contain, it must begin with a first or original stage, or with a theory of social origins. How did Marx document his earliest and original condition among human beings, described as living at the beginning of human history in a propertyless and communal state? From the English historical record? No. From the historical record of any historical people? No.

Since neither the dated historical record four thousand years in length, nor the archaeological record, scores of thousands of years longer, conducts the inquirer as far back in time as the earliest condition of earliest man, some other form of documentation must be used. These documents for most progressionists, as also for Marx, are ethnological materials. By virtue of a complicated argument, it is assumed that the economies or cultures of primitive peoples *now* in existence exhibit that form of social and economic relationships which obtained among the first men on the planet. Without entering upon the formidable infirmities of this argument, an argument now abandoned by most ethnologists, let us examine Marx's attainments as an ethnologist. Of how many nonliterate peoples did he apparently have knowledge? And how did he introduce this knowledge and this argument into the documentation of the materialistic interpretation of history?

Although nonliterate peoples are to be numbered in the hundreds or thousands, Marx mentions in the *Capital* a possible six or seven, including what he calls with amateurish half-knowledge, "the ancient Asiatics," the East Indians, the "wild" Indians of America, the inhabitants of the west coast of Baffin Bay, the eastern Eskimos, and the Kaffirs; his associate, Frederick Engels, extends the list with eight more—the Hawaiians, the Polynesians, the Australians, the Iroquois, the California Indians, the Cherokees, the Malayans, and the Africans. If this be the range of Marx's ethnological information, it is narrow even for the time he wrote in the mid-nineteenth century. Many of his predecessors and contemporaries were far better informed. Nevertheless, these tribal communities are represented by Marx as reminiscent "of the mere animal," as living *today* in a condition like that endured by the first men "at the threshold of history" or "at the dawn of civilization," and separated from the state of things in which today a man brings his labor to market "by an immeasurable interval of time." That the economies of these peoples, like the economies of those for whom there is a dated record, might have undergone many changes and permutations, is a possibility ignored by Marx. Rather it is necessary for Marx to abandon his original hypothesis of the natural and inevitable progressiveness of social change and to assume the *fixity* of these cultures throughout past time. It is necessary to postulate that during past millennia, and while other peoples have been approaching a utopian consummation of their historical experiences, these simple, self-sufficing communities have constantly reproduced themselves at the same cultural level and, "when accidentally destroyed, have sprung up again on the same spot and with the same name."

V

In supporting his philosophy of social progress, then—a philosophy determined by a perfectibilitarian goal known in advance—Marx leaned almost exclusively on English history, and English history during the past three or four centuries rather than the past twenty. As a historian, he relied solely on historical illustrations chosen to fit

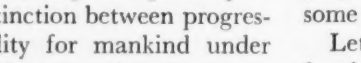
his hypothesis, or on declarative statements—not on the detailed investigation and comparison of histories. He offered merely a handful of ethnological "facts" of a type now repudiated by most ethnologists. Although one of his purposes was to bring the study of man into conformity with the procedures of natural science, or to construct a science of society and history, he disregarded from start to finish a major canon of scientific method, candor. He cited no negative instances. He confessed no difficulties in the verification of his theory. Moreover, like all other philosophers of history, he limited himself to the consideration of a certain group of events in man's past, detached them from their context in the complex tissue of events, and merely arranged them according to a prescribed, a priori scheme. It will be apparent to every student who is confronted with these conspicuous methodological shortcomings in dialectical materialism, or the economic interpretation of history, that Marx's theory of history was not a science of history.

It will also be apparent to the young members of this war-torn generation that the Idea of Progress, as the organizing principle of the Marxist interpretation of history, and the principle upon which the inevitable communization of the world was predicated, can no longer be seriously entertained. In the first place, it is merely a value judgment made three hundred years ago, and its traditional credibility has been shaken by a train of disasters. In the second place, it was made as a value judgment during a more optimistic climate of opinion, and it failed, even in the eighteenth century, to epitomize the historical experiences of all mankind. In the third place, its philosophical proponents have made no distinction between progressive change as a possibility for mankind under appropriate conditions and progressive change as a necessity; between belief in the possibility of progress and a scientifically established law of increasing social perfection. Obviously, such a philosophy of history takes leave of the terra firma of ascertained fact and projects itself into the tenuous realm of speculation and wishful thinking. It becomes divination rather than discovery; myth rather than historical law.

Before the public imposes silence upon the critical discussion of Communist social theory in our institutions of higher learning, it should look well at the kind of evidence offered by Karl Marx in support of the materialist interpretation, as it stands at the basis of Marxism. Is it the kind that could safely be presented in court, before judge and jury? Has it the force and weight with which a scientist could hope to convince other scientists? Is it courting the conversion of college students to Communism to subject Marx's ideas to the same critical scrutiny which is given in college classrooms to Adam Smith, Auguste Comte, Henry Thomas Buckle, or Arnold Toynbee? Or, are students already old enough to defend their country still intellectually too immature to engage in critical analysis?

This, rather than the futile extension of anti-Communist oaths to many institutions of higher learning, is the question which confronts us today—university administrators, college faculties, and the public alike. It should be remembered that young people are idealists and that, without criticism, the easy certainties of Marxism can be made to appeal to their passionate desire for social betterment. It should also be remembered that these easy certainties are being assiduously disseminated outside college walls and university quadrangles. They are taught uncritically, in a popular form, and with the assistance of every device skillful propagandists can lay their hands to. Indeed, to extramural propagandists, no one is too young to respond to some popularized version.

Let us remember that unless the social science faculties of the universities and colleges assume the task of honest, critical analysis of Marx for the benefit of college students, and are not only permitted but urged to do so, there will be no criticism. The extramural propagandists will have everything their own way.



Science will fulfill what is demanded of it only on condition that it retain its freedom and independence in its work. In spite of all necessary limitations, liberty remains an essential characteristic of science, and spiritual and intellectual freedom is a necessity for scientists. Every true scientist desires to serve his nation. His work, however, cannot be complete if it is merely a service to his nation.

—Ferdinand Sauerbruch (Speech at Dresden, September 22, 1936)

SCIENCE ON THE MARCH

INDUSTRIAL LIMESTONE IN INDIANA*

LIMESTONE is a prosaic and unglamorized mineral commodity which rarely impresses the ordinary citizen with its importance, although his dependence upon limestone and the products made from it is constant and direct. The general public is aware of the extensive use of limestone as constructional material for residences, public buildings, bridges, and monuments. For these purposes its use in place of other building materials is a matter of preference and is based upon such factors as appearance, durability, cost, and upkeep. However, for many other important but lesser known purposes no economical substitute exists. Our highway system is heavily dependent upon limestone, both as crushed stone aggregate and as the basic material for the cement which binds our concrete roads. Agricultural limestone, so necessary for successful farming in many areas, is an essential product almost unnoticed by the average city dweller, although its use affects the quality of the food he eats, and the price he pays for it. The iron and steel industry, certainly one of the primary nerve centers of America's industrial coordination, is only one of numerous metal industries which cannot function for a single day without the consumption of huge quantities of limestone.

Building stone. Indiana cannot be termed a typical limestone-producing state, as its building stone constitutes an unusually high percentage of its total limestone production.

Centralization on a grand scale has taken place in the building stone industry during the past fifty years. At the turn of the century at least four limestone formations† in Indiana were commercially exploited for dimension stone. Three of these formations were in use because they occurred as thin-bedded deposits and were usable in their natural bedding thickness. As one dimension was determined by nature, the stone could be fabricated by splitting flat sheets, readily separable at bedding planes, into blocks of the desired width and length. Hand methods in the form of drilling and wedging were adequate for the purpose, although the larger quarries made use of steam power and machinery.

The three formations under discussion, and the areas in which they were developed (Fig. 1), are as follows: A sizable dimension-stone industry existed in Shelby, Decatur, and Franklin counties, Indiana, where the Laurel and Osgood limestones, of Niagaran age, were quarried. Thin-bedded parts of the Levias member of the Ste. Genevieve limestone, upper Meramecian in age, were quarried, principally in Owen County. The stone was used in the form of hand-shaped blocks, which, because of the conchoidal fracture of the rock, possess an appearance readily recognizable in old buildings and foundations in western Indiana and in Illinois. The northern Indiana Niagaran formation, named the Liston Creek limestone, was quarried in Huntington, Grant, Wabash, and Miami counties and is still used on a small scale for flagstones and ornamental purposes. Certain desirable qualities of the Salem limestone in Lawrence, Monroe, and Owen counties have enabled it to supersede all other limestones in the state for dimension purposes, and to become the most widely used building stone in America (Fig. 2). Improvements in machinery and transportation facilities have contributed to the centralization of the dimension stone industry. The formation called the Salem limestone contains the rock commonly termed Indiana Building Stone and formerly known as "Bedford limestone" or "Oolitic limestone." The part of the Salem limestone used for building purposes is massive, even-grained, and composed largely of small fossils and fossil fragments. It shows little tendency to split preferentially and is ideal for milling purposes. Large reserves are available, and new quarries are still being opened.

Crushed stone and agricultural limestone. Glacial drift of three ages is indicated by the three glacial boundaries shown on the accompanying map (Fig. 3) and has a profound effect on the availability of stone for quarrying. The southernmost boundary line marks the maximum extent of the first or oldest of these glaciations (Illinoian). The middle line shows the southern limit of the younger glacia-

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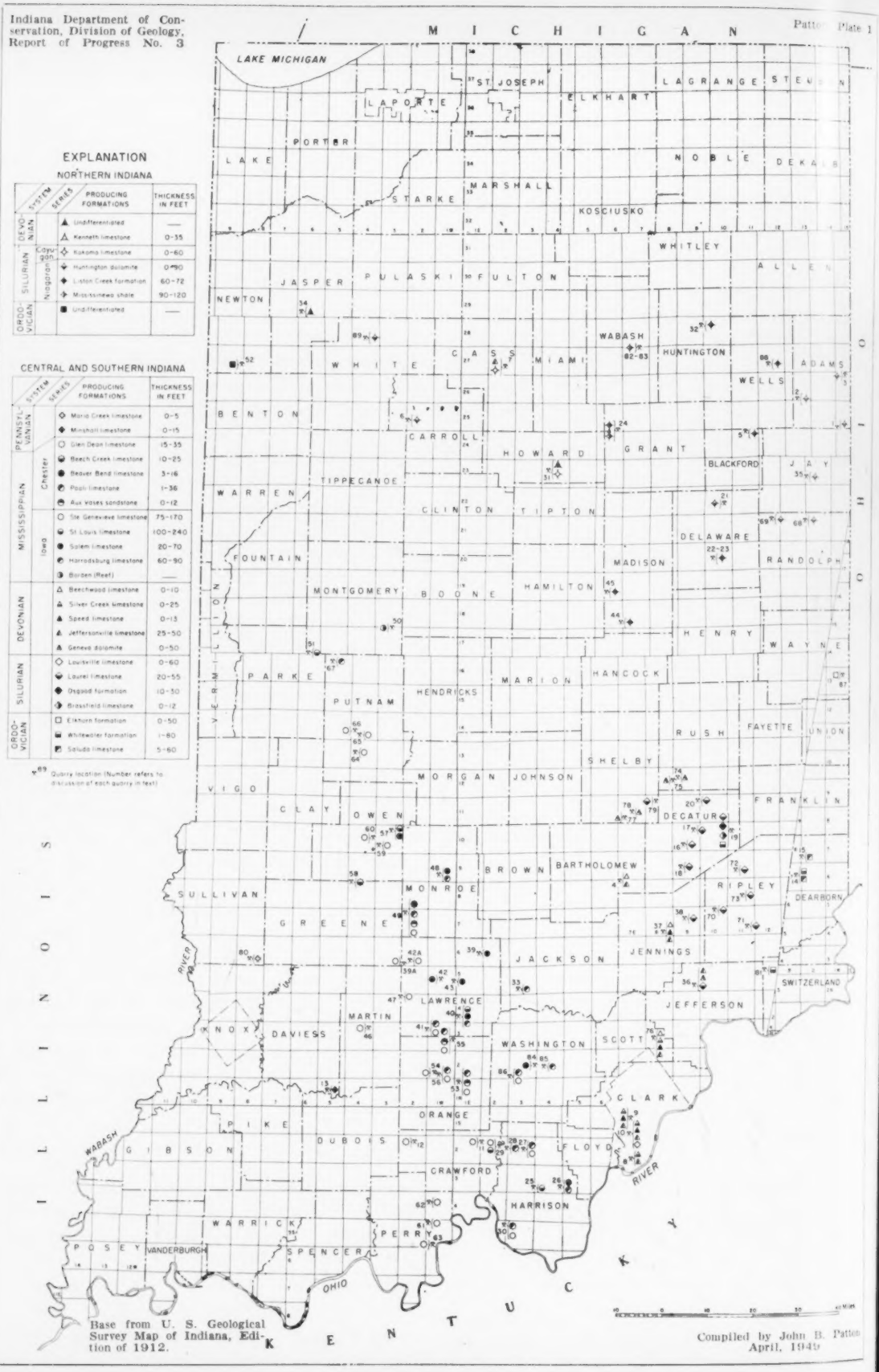
† Formations are named for a geographic locality in which they are well exposed and typically developed. A geologic formation is a mappable unit of the rocks which form the earth's crust and occupies a definite place in the geologic time scale.

Indiana Department of Conservation, Division of Geology, Report of Progress No. 3



EXPLANATION

NORTHERN INDIANA

System	Series	Producing formations	Thickness in feet
Devonian		Undifferentiated	—
		Kenneth limestone	0-35
Silurian	Cayuga	Kokomo limestone	0-60
	Niagara	Huntington dolomite	0-90
		Liston Creek formation	60-72
		Mississinewa shale	90-120
Ordovician		Undifferentiated	—

CENTRAL AND SOUTHERN INDIANA

System	Series	Producing formations	Thickness in feet
Pennsylvanian		Maria Creek limestone	0-5
		Minshall limestone	0-15
Mississippian	Chester	Glen Dean limestone	15-35
		Beech Creek limestone	10-25
		Beaver Bend limestone	3-16
		Pooli limestone	1-36
		Aux Vases sandstone	0-12
	Iowa	Ste. Genevieve limestone	75-170
		St. Louis limestone	100-240
		Salem limestone	20-70
		Harrodsburg limestone	60-90
		Borden (Reef)	—
Devonian		Beechwood limestone	0-10
		Silver Creek limestone	0-25
		Speed limestone	0-13
		Jeffersonville limestone	25-50
		Geneva dolomite	0-50
Silurian		Louisville limestone	0-60
		Laurel limestone	20-55
		Osgood formation	10-30
		Brassfield limestone	0-12
Ordovician		Elkhorn formation	0-50
		Whitewater formation	1-80
		Saluda limestone	5-60

89 Quarry location (Number refers to discussion of each quarry in text)

Base from U. S. Geological Survey Map of Indiana, Edition of 1912.

Compiled by John B. Patton
April, 1949

FIG. 2. A small part of the huge Walsh Quarry in the Salem limestone near Oolitic, Lawrence County, Indiana. In the center of the picture, steam channelers are running on rails and are cutting the limestone into long blocks separated by channels about 10 feet deep. After a row of key blocks has been cut from the long block, split away from the new floor, and removed by the crane, additional long blocks the full length of the channeler track will be tipped over on their sides, cut up, and lifted to rail cars.

tion (Wisconsin), and the northern line indicates the outer margin of the Champaign glacial substage, the youngest of the three.

In the unglaciated area of south-central Indiana and in most of the portions of southeastern and southwestern Indiana which are covered by Illinoian glacial drift, bedrock is close enough to the surface to permit economical stripping and quarrying. Between the outer border of the Wisconsin drift and the lower portion of the area covered by Champaign drift, major streams and some tributaries cut through the glacial drift to bedrock and permit quarrying without excessive stripping. The quarries in Wayne, Rush, Shelby, Bartholomew, Putnam, Montgomery, and northwestern Decatur counties(Fig. 1) are within and near the margin of Wisconsin drift. Thus, in the limestone areas of southern Indiana, potential quarry sites are numerous, and the quarry operator may select a location upon the basis of convenience, geographic advantage, stripping conditions, and character of the limestone. In northern Indiana, however, overburden is thin only in the valleys of major streams and at a few places in the till plains. The operator therefore opens quarries where limestone is available and has little opportunity to select a quarry site from a wide range of possible locations.

FIG. 1. Quarries producing crushed stone in Indiana.

Limestone is not available in the northern tiers of counties, where glacial drift is excessively thick. Silurian limestones are quarried across north-central Indiana, and Devonian limestones are quarried in northwestern Indiana. An unusual exposure of Middle Ordovician limestone, apparently at the surface because of "crypto-volcanism," is exploited east of Kentland, in Newton County. All the limestone and dolomite quarried in northern Indiana is suitable for agricultural limestone; some of the rock makes excellent concrete aggregate and road metal; much of it finds a market solely because it is the only rock within economical hauling distance.

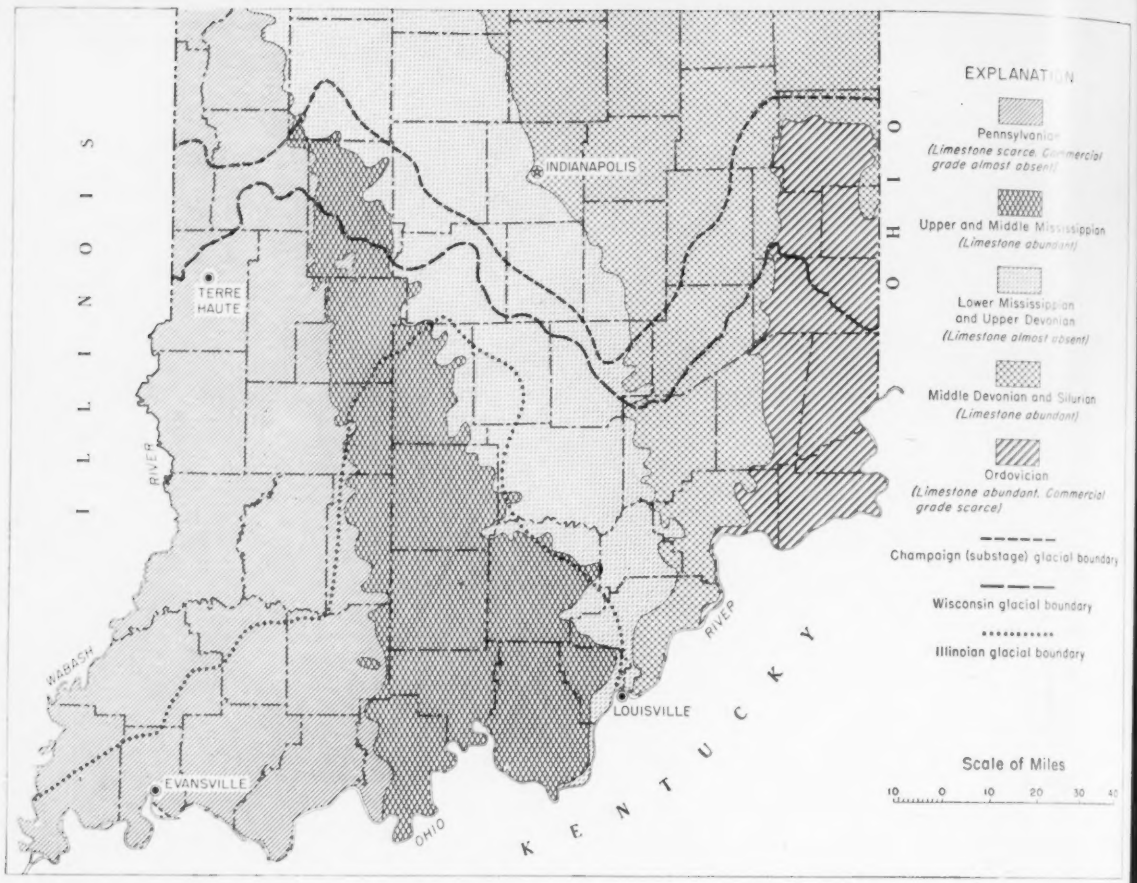


FIG. 3. Generalized bedrock map of southern Indiana.

Figure 3 shows the distribution of bedrock of different geologic ages in southern Indiana. The divisions shown on the map permit certain generalizations concerning the availability of stone for crushing.

Pennsylvanian limestones, although quarried on a small scale in two localities, are not of great commercial importance. Small quarries can exist because they are close to local markets. Careful exploration may discover areas in which certain of the Pennsylvanian limestones are thick enough to permit economical quarrying.

The Mississippian limestones are the most important sources of crushed stone in southern Indiana. The limestones of the Chester series have a geographical advantage in extreme southern Indiana because they are the nearest source of supply to the eastern part of the Illinois Basin. The Mississippian limestones are fairly pure, although the St. Louis and parts of the Ste. Genevieve and Salem are dolomitic. The Ste. Genevieve limestone is the largest single source of crushed stone in southern Indiana. It crops out from the Ohio River northward into Putnam County and is a thick, fairly pure limestone that is well adapted to the operation of large quarries.

Quarryable limestones are not present in the wide region occupied by Lower Mississippian siltstones and shales and Devonian black shale. Devonian and Silurian limestones are quarried in a broad belt from southern Clark County northward into Rush County. The Devonian rocks vary widely in physical characteristics, range in composition from high-calcium limestone to dolomite, and include siliceous and argillaceous limestone suitable for the manufacture of natural cement. The Silurian limestones, except the basal formation (Brassfield), are dolomitic, and the one most widely used (Laurel) is conspicuously flinty. The Cincinnatian limestones in southeastern Indiana commonly contain much disseminated clay and are interbedded with shale. Only by careful selection of quarry sites and, in many cases, by some form of beneficiation, can usable crushed stone and agricultural limestone be obtained from them.

The fines from limestone crushing plants were

FIG. 4. Ste. Genevieve limestone in a quarry near Orleans, Orange County, Indiana. The exposed part of the Fredonia member is 99.0 per cent calcium carbonate. The Rosiclare member is 95.0 per cent calcium carbonate, but would be disqualified for many high-calcium uses by its silica content of 3.6 per cent, present mostly as grains of quartz sand.

unused waste material for many years and presented a problem of disposal. After the use of finely ground limestone for soil treatment became widespread, quarry operators were able to sell all their current production of fines and to sell off their former waste piles. As the liming of soil increased they found it feasible to install hammer mills and other special equipment for producing agricultural limestone. Some quarries now confine their production to this commodity.

Chemical limestone and fluxing stone. Limestones which contain 95 per cent or more of calcium carbonate are termed high-calcium limestones. For some purposes, purity must be even greater. The specifications for each industrial use rule out appreciable quantities of those constituents that would be objectionable. For instance, iron oxide must be virtually absent from limestone which is to be used in making glass.

Large quantities of limestone are used as fluxing stone in smelting and metallurgical refining processes. In the iron and steel industry limestone flux removes phosphorus, carbon, and other impurities and combines with silica from the ore to form a slag which can be separated from the molten metal.

Lime is manufactured by heating calcium carbonate until the carbon dioxide is driven off. Most lime is made from limestone. For some purposes, lime which contains a high proportion of magnesium oxide is desirable, but for most purposes the finished product should be as nearly pure calcium oxide as possible, and the raw material should therefore be high-calcium limestone.

Pure limestone is used in pulverized form as filler for water-base paints, rubber, composition roofing, phonograph records, and many other products. In some cases it is used by the pharmaceutical industry for such products as toothpaste. In animal feeds and fertilizers it acts as bulk filler, furnishes calcium, and may furnish small quantities of other elements if they are present as minor impurities—often desirable ones—in the limestone.

The Ste. Genevieve and Salem contain the chief reserves of high-calcium limestone in Indiana. Other formations, such as the Brassfield and the Devonian Beechwood limestones, are sufficiently pure for chemical purposes but are not thick enough to be quarried alone economically. High-calcium limestone is present in the Ste. Genevieve in the form of lenticular beds of oolite, usually white or light in color, which occur in both the Fredonia and Levias members. Except in underground mining, the beds of oolite and intervening beds of limestone suitable only for other purposes would have to be handled separately. The maximum observed thickness of a single oolite bed in the active quarries of the state is 15.2 feet. Figure 4 shows this bed of Fredonia oolite and the overlying beds of Rosiclare and Levias limestone.

The building-stone facies of the Salem limestone ordinarily contains more than 97 per cent calcium carbonate. Figure 5 shows a massive ledge of building stone (units 1 and 2), and (units 3 and 4) the overlying beds in the quarry. Waste stone from the building-stone mills is used for flux stone or is pulverized for use as chemical lime and for other purposes that require high purity. Although flux stone for use in steel manufacturing is obtained from the

FIG. 5. Quarry in the Salem limestone at Salem, Washington County, Indiana. The massive bed bracketed by units 1 and 2 is the building-stone facies from which the famous Indiana Building Stone is produced. In this quarry the bed has a calcium carbonate content of 97 per cent and would be suitable for chemical limestone. The overlying bed (unit 3) is only 79.4 per cent calcium carbonate, but the top bed (unit 4) is 95.8 per cent calcium carbonate.

Paoli limestone, of the lower Chester series, the Paoli in most areas contains shale partings which decrease its value for this purpose. Northern Indiana has no adequate local supply of pure limestone. Devonian limestone from Cass County has in the past been used extensively as a flux stone but is no longer quarried separately from a thick section of dolomitic stone. The production of chemical limestone and fluxing stone in Indiana is low in contrast to the availability of suitable stone.

Dolomite and high-magnesium limestone. The basal member of the Devonian system in most of southern Indiana is the Geneva dolomite. The overlying formation, the Jeffersonville limestone, although principally a calcium limestone near the Ohio River, becomes progressively more dolomitic in its lower parts northward through its region of outcrop. In northern Indiana, the Huntington dolomite, Niagaran in age, is quarried for general crushed stone purposes. Reefs in the Liston Creek limestone and the Mississinewa shale are highly dolomitized, and, in many places cannot be distinguished from the Huntington dolomite. Dolomite and dolomitic limestone formations of Indiana are not used for refractories nor for any purposes in which high magnesium content is essential.

Portland cement. Limestone is the chief raw material of most portland cement plants. Because of the large tonnage of limestone required, the plants are located near the limestone quarry. Silica, alumina, and iron oxide are necessary constituents and are therefore not objectionable in the limestone unless their amount is excessive. Magnesium content, however, must be relatively low. A quarryable thickness of 30 feet or more of limestone is desirable to permit a high production rate, and the average analysis of the rock in the quarry face must fall within limits which allow the crushed and mixed rock to be fed directly to the kilns. This combination of chemical suitability and practical availability can be obtained at many places in the Mississippian limestones of Indiana and to a more limited extent in the Devonian limestones. Cement plants using local limestone as their principal material are located in Clark County, where the Speed (Devonian) and Jeffersonville limestones are utilized; in Lawrence County, where the Harrodsburg (Mississippian), Salem, and lower St. Louis limestones are used; and in Putnam County, where the Ste. Genevieve limestone is the source of supply.

Natural cement. A large natural cement industry once flourished in Clark County, where the Silver Creek limestone is well developed. Today only one company manufactures natural cement. Parts of the Mississinewa shale of northern Indiana probably are a natural cement rock, as they are similar in chemical composition to the Silver Creek.

Rock wool. The location of an extensive rock wool industry in Delaware, Madison, and Wabash counties was formerly based on the use of the Mississinewa shale as a raw material. Although a large part of the industry now uses slag instead of shale as a raw material, the plants are still in their old locations. The shale is now used as raw material in only one area, eastern Wabash County. High-calcium limestone is used as a flux by the plants utilizing slag.

Changes in needs, processes, and transportation, and the availability of new materials have ended the use of several Indiana limestones. Large reserves of these limestones remain. Combined operations that produce several products from a single quarry will be necessary in many cases, if highest efficiency and maximum use are to be attained.

Figure 6 shows the wall of a Clark County quarry which exhibits a considerable diversity of rock types. The New Albany black shale, which is exposed just beneath the soil cover, is bituminous, and oil has been recovered from it experimentally. The formation is about 100 feet thick, although only the lowermost few feet are present where the picture was taken. Like many black shales, the New Albany contains a wide assortment of chemical constituents and presents interesting possibilities of being combined with some of the underlying limestones to form special products.The Beechwood is high-calcium limestone, but its thickness (about 4 feet) is insufficient for it to be quarried economically alone. The Silver Creek limestone is natural

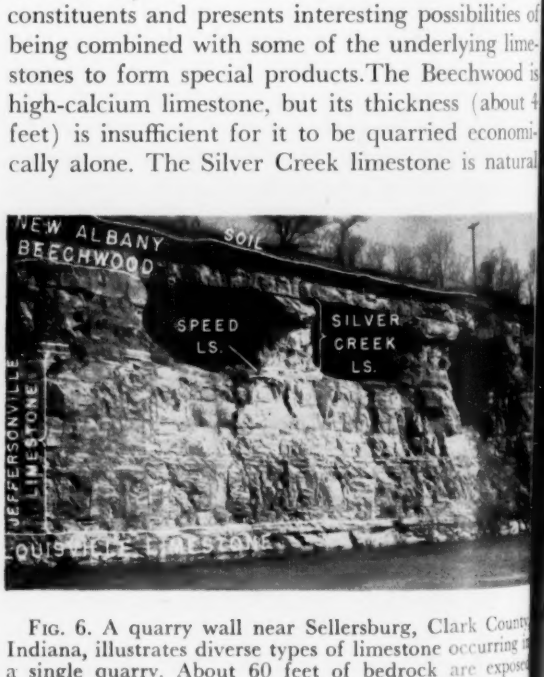


FIG. 6. A quarry wall near Sellersburg, Clark County, Indiana, illustrates diverse types of limestone occurring in a single quarry. About 60 feet of bedrock are exposed above water level.

cement rock, and the tunnels are abandoned underground workings that are relics of the more prosperous days of the natural cement industry. The Silver Creek is argillaceous and is not acceptable for concrete aggregate or agricultural limestone, although it is well suited to the manufacture of rock wool.

The Speed and Jeffersonville limestones are excellent formations for the production of portland cement, road metal, concrete aggregate, agricultural limestone, railroad ballast, filter stone, and most of the other general types of crushed limestone. Only the upper few feet of Louisville limestone are exposed above water level in the quarry, but the formation is about 40 feet thick and is quarried near by. The Louisville can be used for most of the purposes enumerated for the Jeffersonville and Speed limestones, but its higher magnesium content makes it unsuitable for the manufacture of portland cement. This quarry is an outstanding example of the need for selective quarrying. If the entire wall of limestone were blasted down together, the resulting rock mixture would be inferior for all purposes and useless for some. Proper ledging and separation of the rock types would produce several products of high quality.

Some of the carbonate rocks are not used for the purposes to which they are best suited. The production of each type of rock in its most valuable form is both good management and good conservation. Foresighted producers of industrial limestone will probably turn to special products and high-specification materials in their search for economic opportunities.

JOHN B. PATTON

Indiana Geological Survey

New Books Received

Malayan Fisheries. G. L. Kesteven, Ed. viii + 88 pp. Illus. (No price given.) Malaya Pub. House, Singapore. 1949.

The Rise of Scientific Philosophy. Hans Reichenbach. xi + 333 pp. $3.75. University of California Press, Berkeley. 1951.

Commercial Foreign Woods on the American Market. David A. Kribs. v + 157 pp. Illus. (No price given.) Edwards Bros., Ann Arbor, Mich. 1950.

Statement on Race. Ashley Montagu. xi + 172 pp. $2.00. Schuman, New York. 1951.

An Introduction to the Anatomy of Seed Plants. Ernest L. Stover. xiii + 274 pp. Illus. $4.00. Heath, Boston. 1951.

Cloud Physics. D. W. Perrie. 119 pp. Illus. $4.50. Wiley, New York. 1950.

Human Engineering. L. E. Abt, Ed. 154 pp. Illus. $2.75. N. Y. Acad. Sciences, New York. 1951.

Methods of Operations Research. Philip M. Morse and George E. Kimball. vii + 158 pp. Illus. $4.00. Wiley, New York. 1951.

Man Martin. J. C. J. Metford. xi + 154 pp. Illus. $3.75. Philosophical Library, New York. 1950.

Treatise on Heat. M. N. Saha and B. N. Srivastava. (3rd ed.) xii + 935 pp. Illus. Rs. 32/-. Indian Press, Calcutta. 1950.

Geography of the Pacific. Otis W. Freeman, Ed. xii + 573 pp. Illus. $10.00. Wiley, New York. 1951.

Parasitic Infections in Man. Harry Most, Ed. x + 229 pp. Illus. $4.50. Columbia University Press, New York. 1951.

Principles of Phase Equilibria. F. E. W. Wetmore and D. J. LeRoy. x + 200 pp. Illus. $3.50. McGraw-Hill, New York. 1951.

Moses, Who First Saw our Pyramid of Life. A. A. Williamson. viii + 231 pp. $4.75. Philosophical Library, New York. 1950.

The Polarographic Method of Analysis. Otto H. Müller. xii + 209 pp. Illus. $3.50. *Journal of Chemical Education,* Easton, Pa. 1951.

Principles and Practice of Bacteriology. (4th ed.) Arthur H. Bryan and Charles G. Bryan. xv + 410 pp. Illus. $1.75. Barnes & Noble, New York. 1951.

Trends in Gerontology. Nathan W. Shock. ix + 153 pp. Illus. $2.50. Stanford University Press, Stanford, Cal. 1951.

Electric Circuits for Engineers. Edward K. Kraybill. x + 212 pp. Illus. $3.85. Macmillan, New York. 1951.

Quiroga: A Mexican Municipio. Donald D. Brand. v + 242 pp. Illus. $1.75. Inst. of Social Anthropology, Smiths. Institution, GPO, Washington. 1951.

Psychosurgery. (2nd ed.) Walter Freeman and James W. Watts. xxviii + 598 pp. Illus. $10.50. Thomas, Springfield, Ill. 1950.

No Woman's Country; Travels in the Anglo-Egyptian Sudan. Michael Langley. 221 pp. Illus. $4.50. Philosophical Library, New York. 1951.

Geography in the Twentieth Century. Griffith Taylor, Ed. x + 630 pp. Illus. $8.75. Philosophical Library, New York. 1951.

The Impact of Science on Society. Bertrand Russell. 64 pp. $2.00. Columbia University Press, New York. 1951.

A Royal Head from Ancient Egypt. George Steindorff. xv + 30 pp. Illus. (No price given.) Smithsonian Institution, Washington, D. C. 1951.

Health and Human Relations in Germany; Report of a Conference on Problems of Health and Human Relations in Germany. Sponsored by Josiah Macy, Jr. Foundation. 207 pp. $1.00. National Association for Mental Health, New York. 1950.

The Principles of Heredity. (4th ed.) Laurence H. Snyder. xi + 515 pp. Illus. $4.75. Heath, Boston. 1951.

[*Continued on advertising page x*]

BOOK REVIEWS

LOCAL FLORAS DESCRIBED, ANALYZED

The Cacti of Arizona. Lyman Benson. xii + 134 pp. Illus. $4.00. University of Arizona Press, Tucson. (Published at the University of New Mexico Press, Albuquerque.) 1950.

Handbook of North Dakota Plants. O. A. Stevens. 324 pp. Illus. $4.50. North Dakota Agricultural College, Fargo. 1950.

BY PROFESSIONAL botanists and amateurs alike, as well as by geographers interested in the more general aspects of vegetation, the new edition of Benson's *Cacti of Arizona* will be highly prized. To ecologists the unusually detailed (even though small-scale) mapping of distribution cannot fail to suggest correlations with temperature, soil, rainfall, and topography. Countless amateur horticulturists will also appreciate it. Although enthusiasts in the growing of house plants are not relatively as numerous in these days of restricted living quarters as they once were, the cacti continue to hold their own as a favorite group for amateur specialization.

The virtual extermination of rare and localized species in their natural ranges by commercial collectors who supply the amateur trade in wild cacti is to be deplored, but it must be expected that such a detailed guide as Benson's will increase the ranks of that group of persons who know the cacti best but treat them worst.

Even if *The Cacti of Arizona* would inevitably and desirably appeal to intelligent amateurs, its author made no concessions to them in the way of writing on what professionals sometimes consider to be the amateur level. In systematic botany the main difference between the professional and the amateur is likely to be that the former presumably has library facilities which he can be assumed to use, whereas the latter needs books that are reasonably complete in themselves. The student of cacti in Arizona, whether botanically experienced or not, will find in Benson's book all that he will need until he has progressed far. It is perhaps for this reason that it has no bibliography whatever and gives hardly a hint of the existence of the large literature that underlies our present knowledge of the family.

Among the author's more general remarks one curious lapse strikes the reviewer. Benson says: "The only members of the family occurring outside of cultivation in the Old World are a few species of the genus Rhipsalis found in Africa and Ceylon." Actually, prickly pears multiplying as weeds became for a time a major pest in Australia, and *Opuntia* has likewise become so abundant in parts of North Africa that it is characteristic of the landscape.

Benson conceives the specific category broadly, and recognizes only 60 species of cacti in Arizona, with a considerable number of varieties. He has no quarrel with those who split more finely, but believes that, if binomial names are used for somewhat localized geographic entities, a treatment of a local flora employing such names fails to give a correct conception of the position of that flora in the surrounding region and in the world as a whole. There is much to be said in favor of this point of view, and Benson does a real service in stating it clearly. His delimitation of genera is correspondingly broad, and certain generic names established or recognized by Britton and Rose are disused, except as common names for those species of the unrecognized genera which occur in Arizona. These adaptations of generic names as common names are very acceptable, since they are already familiar to, and used by, a host of cactus enthusiasts.

In the matter of choosing other common names, Benson has not invariably been equally felicitous. He has some good and authentic common names, such as "saguaro" and "giant cactus" for *Cereus giganteus* (to which "Carnegiea" is now so generally applied as to be another acceptable alternative common name) and "senita" for *Cereus Schottii.* At the other extreme of acceptability are the clumsy translations of scientific names, now advocated by some as "common names," but of these Benson wisely uses few, preferring to give no common name at all. The reviewer would suggest that for some of the more local varieties and species inquiry might turn up distinctive and interesting names in coextensive Indian dialects. Others might well take the name of the place where they were discovered, or of the Indians of the same area. There could be no great objection to such names as "Papago cactus," "Havasupai cactus," "boundary cactus," etc. Within reason, some very local types for which common names would never be wanted except by cactus enthusiasts, to whom the names of other devotees would be familiar, might even be identified with the names of their discoverers. Benson, for example, is much indebted to three collaborators, A. A. Nichol, Robert H. Peebles, and J. J. Thornber, for aid in his work, and there would be no reasonable objection to *trying* to fix their names in the common nomenclature of the cacti. If, however, the name actually took hold in grass-root vernacular, it is a safe prediction that in a generation they would appear as "nickel cactus," "pebble cactus," and "thorny-burr cactus," and that their use would be extended beyond the intended systematic bounds! If possible, therefore, the imagined word variants should have an imaginary applicability to the species! The most foolish delusion of some advocates of "standardized common names" is that common speech can be standardized and kept that way.

The maps showing geographical distribution with

Arizona of the varieties and species are a valuable contribution to plant geography, and will challenge the attention of specialists to mapping the ranges of the same entities across the Mexican boundary and into other states.

The illustrations, both colored and black-and-white, are scientifically well chosen and well produced. They consist of colored and black halftone plates, and excellent line drawings in the text, the latter done by Lucretia Breazeale Hamilton. The unusually effective and artistic reproductions of color photographs, apparently from five halftone plates for as many colors, are expertly printed, and as decorative as they are scientifically good. Not only botanists but all nature lovers who are interested in the Southwest or in attractive books are advised to secure this admirable little volume before it is again out of print.

It is not clear from the ambiguous imprint whether the University of Arizona Press or the University of New Mexico Press is to be congratulated upon the pleasing format and typography of this second edition of Benson's book, but it is apparently the latter. The imprint is not dated, but the publishers give the actual date of publication (of bibliographical importance because of the publication of new scientific names) as 5 December 1950.

STEVENS' *Handbook of North Dakota Plants* is the first volume in a series being sponsored by the North Dakota Institute for Regional Studies. There is nothing else in print which covers the same ground. As Stevens points out, the area of *Gray's Manual* extends only to the eastern border of North Dakota. The *Manual* can therefore be generally relied upon in the Red River counties, but the present volume will usefully supplement it to the westward. Although approximately 90 per cent of the North Dakota flora may extend eastward into the *Gray's Manual* region, there is a more western element of perhaps 10 per cent which is only covered by Rydberg's *Flora of the Prairies and Plains*, now out of print and not too easily secured.

Stevens' approach to the problem of writing a flora of North Dakota has been that of a botanist in the Agricultural Experiment Station at Fargo for forty years. During this period some 15,000 letters poured in, each requesting the identification of from one to a hundred plant specimens. Since the requests were largely from farmers, the plants they sent were predominantly weeds. As a result of this experience Stevens eventually prepared a bulletin on the weeds of North Dakota. Quite in conformity with this background, the *Handbook* is especially good in its coverage of weeds, to which most floristic authors give too little attention.

Based primarily upon specimens in the herbarium of the Agricultural College, the *Handbook* is vouched for by 20,000 sheets from the region. Since the North Dakota flora, by Stevens' accounting, has 1,143 species, the average number of specimens per species is less than nine. Clearly, this is too few to give an adequate idea of plant distribution within the state. Probably for this reason the mapping of geographic ranges is limited to 61 species having characteristic patterns of distribution, which are shown on 16 small but interesting maps. These indicate that North Dakota botanists with leanings toward plant geography and geobotany have very rewarding opportunities for more detailed work ahead of them.

A remarkable omission is bibliography, for of other regional literature, or more general literature with some specific bearing on North Dakota, there is only cursory, informal mention. Limitations of space and publication funds probably account for the deficiency.

Stevens seems not to believe in the probability of any local endemism in North Dakota. He states that very few species, if any, are peculiar to the state; that no doubt a few native ones remain to be discovered; that probably some may have been destroyed entirely by human occupation; that "the total is probably not much larger than the number now known." With regard to the numerous proposals of "new" species by J. Lunell, an amateur botanist who lived at Leeds from 1894 to 1920, he merely says that most "are regarded by other botanists as minor variations of older species." Nevertheless, they cannot be disregarded as a lot, and in a definitive regional flora each name based upon a type from North Dakota should be critically accounted for, even if only as a synonym.

North Dakota is so large (about 70,000 square miles) that, although forested and water areas constitute only a small part, and, as Stevens says, even if the state has a less diversified topography than many, with much relatively uniform cultivated prairie, there is still room for considerable local floristic diversity. This is indicated by Stevens' map showing "chief physical features," and by the excellent though brief section on "the landscape in relation to plant distribution." Plant distribution can hardly be adequately recorded on the basis of one herbarium specimen for each block of four of the 53 large North Dakota counties, which is now the objective of the herbarium at Fargo. By cooperation of the North Dakota Institute for Regional Studies not only with the Agricultural College but also with the State University, the several normal colleges, and the private colleges of the state, it should be possible to collect more adequate materials for such a bibliographical enumeration as that suggested. Stevens has clearly shown where the chief gaps in knowledge lie.

One reason why regional studies are not more often produced is that it is hard to find publishers for them. It augurs well that North Dakota realizes what needs to be done and has taken the problem in hand through its Institute for Regional Studies. Stevens' book makes a good start in its program of publication. From the standpoint of bookmaking the volume is somewhat jobprinterish. Although the author and state contract printer did not have the advantage of professional editorial advice, they did not do too badly on the whole. The chief idiosyncrasy in typography arises from omitting (for reasons of economy) the descriptions of the less represented genera. So there is a general key for each family, which leads to species for the smaller

genera but only to the genus for the larger ones. The species of a whole family are numbered consecutively, but the logical and typographic continuity of the text is broken by interpolating those generic descriptions and keys which are retained, with their boldly printed, centered, widely spaced headings, into the sequence of closely spaced specific descriptions.

H. H. BARTLETT

Botanical Gardens
University of Michigan

ZOOLOGICAL ADVENTURE

Dragons in Amber—Further Adventures of a Romantic Naturalist. Willy Ley. viii + 328 pp. Illus. $3.75. Viking, New York. 1951.

THOSE who think that science writing is dull and dreary, full of data and sesquipedalian terminology, should read Willy Ley. Here is a storyteller of natural history's past and present, who can make the very stones beneath our feet come to life as he marshals the facts of science in unusual fashion. He does this with such charm that country weeklies pick up bits from his writings for "filler copy."

Dragons in Amber is a heterogeneous collection of facts, fables, and fancies about practically everything that lives or once lived under the sun. The whole has been put together to make a book that can be opened almost anywhere and provide entertainment and education. It might be called a sequel to his previous work *The Lungfish, the Dodo, and the Unicorn.* But if both books were included within one cover the collection would still make sense under either title.

In fact, if you look up "dragons" in the index of this latest work you won't find them listed at all, in spite of the book's title. On the other hand, there are three such listings in Ley's earlier excursion into romantic zoology. By "dragon" he really means lizard—but don't bother to look up lizard in the index either, because it isn't there. Two chapters, at least, will have to be searched before the reader finds the answer to what might be called "The Case of the Amber Lizard." Only one vertebrate animal has ever been found in amber. It turned up in a European collection in 1875, complete except for the tip of its tail. The tiny creature measured less than two inches in length and has been classified as almost identical with a modern type of lizard native to tropical and southern Africa.

The author pursues his subjects, whether dinosaurs, cycads, permafrost-hidden mammoths, or climbing fish, with the techniques common to both natural science and crime detection. Some of the details told about amber and the inclusions found therein are well known to anyone who has ever had much interest in the subject. But, on the other hand, even old amber-hounds whose loving hands have smoothed many an amulet, or caused one to discover its faint, characteristic, resinous odor, will find references here that are both ancient and novel.

Dragons in Amber is just the sort of book one might expect of a scientist trained with typical German thoroughness in zoology and paleontology; also of one who is appreciative of the long, hard road by which mankind traveled in search of scientific truth regarding the earth's flora and fauna. If Ley dwells rather lengthily at times upon the fictional contents of past teachings, it would seem pardonable in a book of this sort, for, after all, he is telling ageless stories where sometimes truth is still hard to separate from fiction. Yet there are very few instances where the technical critic can point to "tall tales" that are not plainly so labeled.

HERBERT B. NICHOLS

U. S. Geological Survey
Washington, D. C.

IMITATION OF NATURE

Crystal Growth. H. E. Buckley. xv + 571 pp. Illus. $9.00. Wiley, New York; Chapman and Hall, London. 1951.

INTEREST in the growth of crystals has increased enormously during the past five years; therefore, scientists in general, and specialists in the field of crystal growth, should welcome this publication. It is the first of its kind in English to give a comprehensive survey of the useful information found in the literature on this subject.

Most workers in this field sooner or later become acquainted with a series of papers by H. E. Buckley, published about twenty years ago in the *Zeitschrift für Kristallographie.* These, and others of more recent date, deal mostly with crystal habit, the modifying effects of impurities, and with peculiarities of crystal growth. Although the author places considerable emphasis on this portion of the subject, particularly in the last three chapters and a valuable reference appendix, the book is not out of proportion in this respect.

The book opens with a discussion of one of the most important phases of crystal chemistry, solubility of crystals and its dependence on particle size, together with an explanation of the metastable region of supersaturation in which crystals are effectively grown. Following this there is a thoroughly up-to-date section on methods of crystal growth. A new reciprocating rotary apparatus is described for growing them from solution. This apparatus should prove useful for growing many kinds of crystals in colleges and industrial laboratories.

Several chapters are on various aspects of the theories of crystal growth. There may be some question whether so much space should be given to the discussion of the surface energy theory of Curie, and the ideas of Balarev, Traube, von Behren, and others, but there is no doubt that such material helps to give perspective to the problem. The treatment of the more acceptable theories of Kossel and Stranski is very well done.

There is still too much art and not enough science in the growing of crystals. One difficulty in reconciling the two may be appropriately illustrated by the story of the blind men describing an elephant. Each felt a different portion of the beast and each described a totally different thing. Factors involved in the nature of crystal growth are so numerous and varied that generali-

zations from even a fairly large number of observations are apt to be incomplete. It is the conclusion of some workers in this field that a perfect crystal will not grow, surface imperfections being necessary to start growth. Others find such a viewpoint difficult to accept. Most crystals contain imperfections, particularly those found in nature, but useful sections may be obtained from them. So also may one consider this book.

A. C. WALKER

Bell Telephone Laboratories
Murray Hill, New Jersey

EXPLORING THE ROCKIES

Rocky Mountain Naturalists. Joseph Ewan. xiv + 358 pp. Illus. $5.00. University of Denver Press. 1950.

THIS is a guide to the naturalists of the Rocky Mountains—who they were, when and where they lived, and what they did to advance the biological sciences in the West. Of the 343 pages of text, 116 consist of chapter-length biographies of nine of the naturalists, 20 pages are notes and references thereon, and 207 pages are a roster of 798 natural-history collectors in the Rocky Mountains, with brief biographical notes on each. Both the nine biographies and the roster show the mark of a vast amount of patient research on the part of the author. Not only have conventional biographies been consulted, but also family papers, archives of historical societies, newspaper files, hotel registers, and even more obscure sources.

The book opens with a chapter on Avenues of Penetration to the Timpanagos and the North Pacific—an Historical Review, which details the early history of European exploration and penetration of the Rocky Mountains and its impact on the natural sciences of the time.

Of the biographies that follow, perhaps the most notable is that of T. D. A. Cockerell, which sets forth with loving care the life, times, and work of a naturalist of the old school. The reader sees the naturalist's cluttered workroom, learns his varied interests and enthusiasms, and gains an insight into the inspiration he must have imparted to his younger associates in the natural sciences.

Some notion is given of the fascination that the high peaks, the foothills, and the plains must have had for scientific visitors from the East and abroad during the last century, when the country was new and unfamiliar. It is interesting to note the number who, while retaining their homes and occupations in the Middle West, were drawn summer after summer to the mountains. A high point of the narrative is the account of the visit of Asa Gray to Colorado in 1872, and of his ascent of the newly named Grays Peak, in company with Mrs. Gray, the naturalists Parry and Greene, and various local citizens. Other memorable scenes stand out—such as the chance meeting in the Denver railway station of the young naturalist Greene and the old naturalist Torrey.

Some of the other figures do not quite come to life. Edwin James is described as the "first white man to ascend a 14,000 foot peak in America," but when one comes to the actual moment of ascent (of Pikes Peak, during Major Long's expedition of 1820), we are told that "there is no need to relate his story of the ascent, so well told by himself." The general reader, without easy access to the original account of the expedition, is left disappointed.

The author, in a modest disclaimer, describes his book as "materials toward an account of the lives and travels of Rocky Mountain naturalists." The field is vast, and the title is more comprehensive than the actual subject matter. The area dealt with is mainly the Rocky Mountains in Colorado, and the naturalists treated are mainly taxonomic botanists. Less attention is given to other parts of the Rocky Mountains, and to the explorations of zoologists, geologists, and others.

The roster of natural-history collectors, in the last half of the book, covers a wider range than the preceding biographies; one notes not only the names of botanists, but also of geologists, paleontologists, and geomorphologists. These are, however, peripheral to the author's main interests. The reviewer (as a professional geologist) wonders why use was not made of the memorials of the Geological Society of America,* a standard source of biographical data on geologists, and why the 1906 edition† of Merrill's history of American geology was used, rather than the more complete 1924 edition.‡ Of the three authors of the Denver basin monograph,§ a classic of Colorado geology and natural history, only one (Cross) is listed. The various bureaucratic posts held by William H. Holmes are enumerated, but his most enduring claim to fame is not mentioned—his vivid line drawings of Western scenes and terrain, that embellished many of the reports of Western territorial surveys.

Perhaps such breadth of treatment was not the aim of the author, but the lack of it tends to detract from the interest of the book for the general reader, and to restrict its field of usefulness to a smaller audience of specialists. The book does point the way toward a subject worthy of continuing investigation—not merely of Western adventure and history on the one hand, nor of botanical exploration alone on the other, but of the full sweep of discovery in the natural sciences in the new land of the West that was being opened up during the last century.

PHILIP B. KING

U. S. Geological Survey
Gatlinburg, Tennessee

* In *Geol. Soc. Am. Proc.*, published annually.

† Merrill, G. P. *Contributions to the History of American Geology. Smithsonian Inst., Ann. Rept. 1904,* 189–733 (1906).

‡ Merrill, G. P. *The First Hundred Years of American Geology.* New Haven: Yale Univ. Press (1924).

§ Emmons, S. F., Cross, W., and Eldridge, G. H. *Geology of the Denver Basin in Colorado. U. S. Geol. Survey, Mon. 27* (1896).

NEW CLUES TO INSECT BEHAVIOR

Bees: Their Vision, Chemical Senses, and Language. Karl von Frisch. xiii + 119 pp. Illus. $3.00. Cornell University Press, Ithaca, N. Y. 1950.

THIS little book contains, for the first time in published form, the text of three lectures given by the author in the spring of 1949 during a three-month tour of the United States. The trip was supported jointly by Cornell University and the Rockefeller Foundation, and was finally extended to include the American Museum of Natural History, the University of Minnesota, and a number of other universities and scientific institutions. Those who had the good fortune to be present at the delivery of these lectures will find both pleasure and profit in being able to review them, and it is indeed fortunate that the various problems discussed therein are now made available in this form to a wider circle.

In addition to foreword and preface, there are the three lectures dealing, respectively, with the color sense of bees (pp. 1–24), the chemical senses of bees (pp. 25–51), and the language of bees (pp. 53–96). These are followed by an appendix, in which the original lectures have been supplemented by additional material providing more detailed accounts of crucial experiments and of more recent investigations carried on by the author. There has also been included a most useful bibliography of 58 titles, ranging in date from 1908 to 1950, inclusive, and making reference to the work of 29 writers, among them Autrum, Bauer, Forel, Haslinger, Hertz, Lotmar, Porsch, Schaller, and Vogel.

For the benefit of those who may not be familiar with the general type of investigation reported by von Frisch in this book, it may be stated that he undertakes to give a brief résumé of some conclusions reached by him after more than forty years of experimental work in animal behavior. These conclusions are based largely on tests made by himself and, under his direction, by his staff, on reactions of honeybees. In so doing, the author has made a special effort not to present his readers "with vague or mystical speculations, but rather with phenomena . . . which are concrete and readily observed." This attribute has been brought out and felicitously emphasized in the foreword, prepared by Donald R. Griffin, of the Department of Zoology of Cornell University, who has greeted with enthusiasm the publication of the lectures.

As would be expected, the discussion of the color sense of bees deals first with the cooperative relationship between bees and flowers, whereby the insects obtain food, and the plants achieve the transport of their pollen. Then follow narrations of results of experiments relative to the ability of bees to distinguish certain colors and difference between solid and broken patterns. These studies, particularly those in the form of flowers, led to the discovery of means other than color and form by which bees could recognize flowers—notably by their scent, a characteristic of each flower. These were followed in turn by studies in the location and degree of discrimination of the organs of taste and smell of the honeybee, the results of which are duly recorded under the chemical senses of bees. Of particular interest and of considerable ingenuity were those tests von Frisch made to determine the ability of bees to distinguish between several different smells, and also those of the so-called threshold of perception, apparently the lowest concentration that will stimulate the sense of taste—not only of sweet, but of salty, sour, and bitter tastes as well. Some practical applications of the knowledge gained are cited, such as where used by various scientists in improvement in the pollination of red clover—excellent examples, by the way, of that well-known fact that research work performed for its scientific interest alone often proves later on to be of great practical value in ways that never could have been foreseen.

It is, however, the lecture dealing with von Frisch's experimental work in the language of bees that is perhaps the most interesting, in that it is a nearer approach to our borderline of new knowledge. Regrettably, space limitations forbid detailed description here of the cleverness of contrivance and the ingenuity, as well as the long-continued observation, by which he was enabled to arrive at his conclusions. It may be said, however, that through use of glass-walled observation hives and of bees individually identified by paint spots of various colors, the observer was able to watch scout bees go out and return from foraging, and in their own way to communicate their findings to their fellows. Apparently this is done by various well-defined series of gyrations, or "dances," to indicate direction and distance of food in some relation to the position of the sun. Interpretation by the other bees is sufficiently accurate to enable them to find the places previously visited by the scouts, although sometimes these are as far as three miles away from the hive.

The behavior of honeybees is reminiscent of the age-old mystery behind the commonplace fact of the migration of birds, and recalls to mind that there is much else in the behavior of the lower animals not to be explained as simple automatic responses. And, when studies like these are put on record, then it is that we realize more keenly something of the enormous enrichment of human thought that has resulted over the years from the patient, far-sighted and well-organized research by men of science, and—more important—they open up to us wider vision and clearer comprehension of this vast and complicated universe in which we live.

J. S. WADE
Department of Entomology and Plant Quarantine, USDA
Washington, D. C.

LETTERS

ISOLATING THE SYSTEM

DR. PRESTON'S article "Gas Laws and Wealth Laws" in the November 1950 issue of THE SCIENTIFIC MONTHLY startled me by its brilliant drawing together of similarities in the behavior of molecules and people. So convincing was the analogy that I shivered at the prospect of absolute economic zero.

Analogies, useful as they are, are not without their pitfalls. Agreeing wholeheartedly with Dr. Preston that "We might do far better to try to understand the law . . ." it occurred to me that one of the basic concepts I learned in studying the "laws of nature" was that of *isolating the system* under consideration. Water runs downhill in the universe at large; within an isolated system, such as a siphon or hydraulic ram, may it not run uphill? Almost a century ago, Darwin's "natural selection" was seized upon by certain economists to justify a laissez-faire economy. Today, although recognizing the tremendous value of Darwin's hypothesis to our understanding of a large segment of organic nature, many of us would consider it naïve to apply this "law of nature" baldly to human society as an isolated system.

John Burroughs, in *Man and Nature*, has defined this position far more beautifully and clearly than I could hope to do:

> Nature knows no economy. Nature's ways are not our ways. The rivers drain the hills and mountains and still never run dry. She trusts her seeds to every wind that blows, and to the foot of every bird, no matter which way they are going. And in time her marsh gets planted. She is blind. Her method is the hit-and-miss method of a man who fights in the dark. She hits her mark, not because she aims at it, but because she shoots in all directions. She trims her trees and thins her forests, or reforests her lands in the most roundabout, dilatory, and inefficient manner.
>
> But behold how she has endowed man to improve upon all her slack and roundabout methods. Man sees where Nature is blind; he takes a short-cut where she goes far around. She enables him to cheat, and mislead, and circumvent her. He steals her secrets, he tames her lightnings, he forces her hand on a hundred occasions. He turns her rivers, he levels her hills, he drains her marshes, he makes her deserts bloom as the rose. . . . Nature, working in man and through him, has made the world more fit for man's abode.

Let us increase our understanding of the laws of nature, share illuminating insights among different realms of inquiry, but let us remember the boundaries and conditions of the system we are talking about.

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MINOR KEYS AND MAJOR OPERATIONS

MUSIC with anesthesia has been installed in the University of Chicago clinics to help alleviate tension of patients undergoing surgery. Used with spinal, local, or regional anesthesia, the music is piped to the operating rooms from a central recorder room where three duo-channel magnetic tape recorders of classical, semiclassical, and popular music play continuously for four hours. The patient and the anesthesiologist, who proctors the program, hear the music through lightweight stethoscope-type earphones.—*Science*, **111**, 683 (1950).

> Once they offered sleep and no pain
> With a whiff of cyclopropane,
> But now there's more than that to ease yuh—
> Aoide pipes for anesthesia. . . .
>
> Come, doctor dear, put out the light
> To strains of Bach and Horace Heidt;
> To sweet oblivion turn me loose-y
> With Spike Jones, Handel, or Debussy.
>
> Ere knife deletes among the vitals,
> Reduce my tension with recitals!

VIVIEN MORGAN HONE

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PERILS OF RESEARCH

COLONEL WALKER, in proposing an automatic electronic library ("Blueprint for Knowledge," *Sci. Monthly*, **72**, 90 [1951]), appears to believe that research deals mainly with standardized concepts expressed in standardized terms, and that most references are written in English and have fully informative titles. If such a standardized, static world ever came into being, perhaps research-worker John Doe could indeed sit by his viewing screen and dial "sex life," "elephant," and "India," to have displayed before him all works which could contribute to an analysis of the sex life of the Indian elephant.

But may I compare my dream with Colonel Walker's? In my dream, also, the machinery performs perfectly, but the effectiveness leaves something to be desired. The very first reference flashed before Doe's eyes is a 400-page book called simply *I Remember*, by Sir Namby Pamby; the table of contents sounds somewhat pertinent, but there is no index, and ten minutes are wasted in scanning the entire book forwards at high speed and then backwards at low speed, with no success. Doe shrugs his shoulders and goes on to the next reference, a book called *India and her Animals*; it contains material on sex life (of frogs), elephants (recent bone measurements of), and India (variability of rainfall), but nothing dealing *simultaneously* with sex life, elephants, and India.

After wasting only fifteen minutes on this, Doe rests his eyes a few minutes and then gamely presses the button for reference No. 3—which is in Russian. References Nos. 4 through 50,000 are found (in the course of only six weeks of eyestrain) to consist mainly of

German, Russian, Japanese, and Hindustanese articles; the remainder consist mainly of articles which, although written in English and containing mention of elephants' sex life, prefer not to discuss this subject in specific detail. Perhaps 5 per cent of the articles (2,500 of them) might be found to contain some pertinent information if one had the time to read them carefully; the two most promising references are the *Encyclopaedia Britannica* and a textbook written by Doe himself ten years before.

The final image to flash on the screen has a devastating impact: it is a concluding message which says: "Re 'sex life,' see also the following 20 subjects: courtship, fertility, reproduction, breeding, family life, . . . Re 'elephant,' see also the following 15 subjects: *Elephas,* pachyderm, *Loxodonta,* . . . Re 'India,' see also the following 10 subjects: New India, Greater India, Hindustan, Bhutan, Nepal, Bengal, Pakistan, . . ." Doe, it appears, has covered only one combination of headings out of a possible $20 \times 15 \times 10$, or 3,000. His bill, computed at the nominal rate of $5 per hour, already amounts to $1,200.

Even if Colonel Walker should persuade 10,000 scientists to stop their work and devote ten years to building the hundred-billion-dollar automatic library, the effort cannot but fail. Research men, more often than not, are experimenting with previously unthought-of approaches to previously unthought-of problems, and recording their results by means of newly coined words whose connotations change from year to year and from country to country. No machine, no matter how expensive, can really fill the need here; the ideal instrument—which must be capable of continuously reviewing, revising, renaming, resorting—is the human brain itself, aided by thoughtfully prepared monographs, indices, etc., of reasonably limited scope.

WILLIAM A. SHURCLIFF
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DISTRIBUTION OF DISEASE

I HAVE read with interest "The Geography of Pathology," by Jacques M. May, which appeared in the February SCIENTIFIC MONTHLY. This is a timely article and deals with a subject which has long been neglected by investigators in the medical fields of research. An excellent opportunity was lost in studying the geography of disease when statistical studies were not included in the cooperative health and sanitation programs that were initiated throughout Latin America in 1942. Certain workers, however, on their own initiative, carried on local studies that have rendered valuable information in this field.

In addition to the pattern of certain communicable diseases pointed out by Dr. May, it might be interesting to call attention to pertussis, or whooping cough, which is the most serious disease of childhood and infancy throughout the central highlands and eastern slopes of the Andes. It is not unusual to see an epidemic, within a few weeks, cause the death of half the young children in some of the villages in the region. In other words, the virulence of the disease in this area appears to be about ten times that in the United States. This is an example of Dr. May's reference to geographical distribution of variations of the same disease.

Another would be the difference in crab yaws as found in Haiti and eastern Colombia. The term crab yaws refers to the involvement of the feet. In Haiti these lesions are usually the type known as dry crab, since they consist of lesions of various depths which are relatively dry, whereas in eastern Colombia the lesions usually are suppurating and inflammatory in character.

Perhaps the most spectacular disease of sharp geographic limitation is the highly fatal Carrion's disease, or *verruga peruana.* This exotic scourge killed so many of Pizarro's soldiers that it was called for a time the disease of the conquistadors. In recent years (1870) 7,000 workmen died during the construction of the railroad to Oroya and the copper mines. This gave it the name of Oroya fever.

The disease is caused by small germlike organisms called Bartonella that grow on the red blood cells, and is transmitted by the *Phlebotomus,* a biting fly so small that it can pass through the ordinary mosquito net. Strangest of the many unusual facts about Carrion's disease is its definite and narrow limit to the *quebradas* ("canyons") of twenty-one small rivers which empty along the Peruvian coast, and tributaries of the Marañon, whose waters eventually reach the Amazon. Altitude further limits the disease by confining it to heights between 500 and 3,000 meters. Despite the fact that *Phlebotomus* occurs over the majority of the South American continent, Carrion's disease has escaped these narrow confines only once, when a few cases appeared in Ecuador and Colombia in 1939. It is only transmitted at night.

One of the most serious diseases of doubtful etiology is infantile cirrhosis of the liver. This condition both in the New and the Old World causes many deaths in certain tropical areas. Its distribution as well as its treatment is receiving a great deal of serious attention at the present time.

Greater value may be found in geographical distribution studies of noninfectious diseases. The North American population is with reason greatly interested in diseases of the heart and circulation, with the result that an enormous amount of money is being furnished for research in this field. So far results have been slow in appearing from this research, yet it is known that in certain zones in our southern continent the residents do not develop pathology of the heart and blood vessels. It might be suggested, therefore, that a profitable study could be made in these areas to determine if possible by what means residents are protected from these chronic and fatal conditions. It is true that investigators undertaking such a study would have to live in surroundings where they would not have access to many of the comforts of pampered civilized life, and this perhaps partly explains the lack of enthusiasm for undertaking such a project.

I appreciate Dr. May's reference to high-altitude man. Work on this subject that is being carried on under the direction of Carlos Monge M., of Lima, has and will furnish valuable information on the physiology and adaptability of the human organism. I acquired the 8 million red cells per cubic centimeter referred to in the article while I was living in the Andes, and the subsequent adjustments upon my return to low altitudes were extremely diverting. As to the disastrous effect of changing altitudes rapidly, no better example can be cited than the Bolivian soldiers from the Altiplano who were used in the lowland Choco war. This radical change in environment greatly reduced the number of effective soldiers in the Bolivian Army. Another effect of high altitude on man's physiology is the bradycardia that is found among permanent residents above 11,000 feet elevation. It is not unusual to find a "normal" heartbeat below 50, and most of the population have a heartbeat that averages 20 beats below those in this country.

Care must be taken in drawing conclusions from results obtained in geographic studies of disease. As an example of this it might be well to draw attention to the assumption recently reached following the discovery that the incidence of dental caries was very low in a certain county in Texas. Upon examination it was found that the fluorine content of the water in that area differed from that in other parts of the country, and it was concluded that this fluorine in the water protected the residents against dental caries. This resulted in a large part of the population of the United States drinking fluorine cocktails either privately or as furnished by their public water supply.

Since then two locations have been found in Brazil where the absence of dental caries is even more pronounced than in Texas. On examination, one of these regions does prove to have a certain amount of fluorine in the water, but the other locality uses water where fluorine is practically absent. Compare these with another area in Brazil, located in northern Minas Gerais, that suffers from massive dental caries. Knowing the territory was highly mineralized and contained many fluorite mines, I gathered samples of water and had them analyzed. Fluorine was high in all waters examined, and even the river water contained 2 ppm of fluorine. Therefore we see that in studying the geography of disease such a study must be complete, and conclusions of cause and effect must be carefully analyzed and proved before they can be accepted.

EUGENE H. PAYNE, M.D.

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NO PANTHEIST

READING your February issue, I came upon a book review entitled by you "Pantheistic Forestry." What slush that must be, I thought. And then I discovered, between laughter and dismay, that that was what you had made of my recent book, *A Natural History of Trees of Northern and Central North America*. In no sense is mine a work on forestry as that science is understood today. And I am not a pantheist at all. Your distinguished reviewer, Dr. Ewan, also calls me a "stout pantheist." As I understand pantheism it is the belief that God is nothing but the combined units of our physical universe—God is in the hydrogen atom and the second law of thermodynamics, in cockroaches, and in the interior of Canopus, as much as he is in the fairest flowers or the noblest aspirations of men. I admit I have never found out just what my religion is, but I have no conscious pantheistical leanings nor do I think they can be found in, or read into, my studies in the sylva of North America.

It is such an honor to be reviewed in THE SCIENTIFIC MONTHLY, and your review of my book is so long and generous both in uttered and implied approval, that perhaps I should pick up my winnings and leave the table while luck is still with me. But I care, as I could never care for the opinion of any other audience, what your readers think. So I am constrained to protest, in friendly spirit, against some of the loose statements of Ewan's review.

In paragraph two of his review, Ewan informs his readers that "some of the stories are highlighted with at best only good historical guesses. Much of the detail for the travel and times of Michaux is still very incomplete, and any conclusion as to where and when such and such a tree was discovered is yet to be documented from a broad synthesis of evidence." It is a somewhat serious thing to charge a scientist with guesswork, and I think if Dr. Ewan will look again he will see that when I give an exact place and date for a collection by Michaux, for instance, I have quoted directly from his diaries, which show him as knowing where he was and what the date. In cases where we do not know precise dates and locales of discovery, I give none. When the whole thing is conjectural, I say so.

In a footnote Ewan states that I have no idea of the ecological succession of one tree species by another. I discuss ecological succession in many places—the trembling aspen, the jack pine, the hemlock, the gray birch, the bur oak, and others. Ewan's charge seems to rest on a single word in the last paragraph of my book. That word is "balance." We differ as to the way it is to be employed. But, even supposing I am wrong, the rest of the book shows that I have at least *heard* of ecological succession. I happen to have been a student of Cowles.

Ewan calls my book semipopular, a phrase that always sounds to me as if it meant "too simple for the scientists and too steep for the populace." I intended to make these essays *completely* popular. On the other hand, the descriptions of genera and species are strictly scientific, and so are the keys and synonymy, which is far more extensive than that for the arborescent species in *Gray's Manual*, eighth edition. On the other hand the glossary, I am glad to state, is much shorter. It is as easy to employ a lengthy vocabulary of technical terms as it is to spend money; it takes much judgment and self-denial to retrench within an abridged technical vocabulary. The keys to species were mimeographed and tried out on summer botanical courses at the Uni-

versity of Wisconsin, and then revised in accordance with the difficulties encountered by the students.

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FULLER EXPLANATION

IN ANSWER to the questions of your correspondents, I have pleasure in making the following explanation of the picture used on the January cover of THE SCIENTIFIC MONTHLY.

This photograph illustrates an interference phenomenon that arises in the Schlieren system under particular conditions of illumination, namely monochromatic light and a narrow light source. The phenomenon was reported in the July 1949 issue of the *Journal of the Optical Society of America*. Technical data for the photograph include: Film, Triple S Pan; exposure time, 1/25 second; light source, GE type H-4 Mercury lamp focused by condenser through Wratten 77A filter upon spectrometer slit .03 mm wide; mirrors of Schlieren system in conventional Z-arrangement; mirrors 12-inch diameter, 72-inch focal length; object, a lighted candle placed midway between the mirrors in the collimated beam; camera of adequate bellows extension, using a single element of a convertible anastigmat of 20½-inch focal length placed beyond the knife edge and focused through the second mirror upon the candle; and last, in explanation of the bilateral Schlieren, a wire .005-inch in diameter to replace the usual knife edge. The candle was illuminated by a separate lamp, which explains the realistic appearance of the wax where there would otherwise have been a silhouette.

In explanation of the interference fringes which appear in the Schlieren, it is proposed that light deviated by the refractive gradient in the convection current above the candle falls upon regions in the camera image that are illuminated by undeviated light. Where the path lengths of the deviated and undeviated portions of the beam differ by ½ wavelength, destructive interference results in a darkening in the image. The number of fringes has been shown to depend upon the wavelength of the light, the length of the path through the heated air, and the refractive index gradient (temperature difference in the air column).

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HISTORY OF SCIENCE

A CURIOUS statement appears in William Herbert Hobbs' article, "The Fourteenth-Century Discovery of America by Antonio Zeno" (*Sci. Monthly*, **72**, 23 [1951]): "The explorers had, of course, not taken latitudes and had never even mentioned them, the astrolabe having been brought into use to fix latitude a century after their time" (i.e., 1390).

This occasions some difficulty in view of Chaucer's *Treatise on the Astrolabe, ca.* 1391, which he wrote for "Lyte Lowys my sone" who, though but ten years of age, had been begging his father to instruct him in the theory and use of this "noble instrument." In the *Prologus* Chaucer tells his son that Part III of the *Treatise*

> . . . shal contene diverse tables of longitudes and latitudes of sterres fixe for the Astrelabie, and tables of longitude of citees and townes; and tables as well for the governaunce of a clokke, as for to fynde the altitude of a meridian; and many anothir notable conclusioun after the kalenders of the reverent clerkes, Frere J. Somer and Frere N. Lenne (*The Complete Works of Chaucer*, edited by F. N. Robinson, p. 642. For the Skeat Oxford *Chaucer*, p. 397).

Chaucer's detailed treatment of latitude and longitude determination will be found in Part II, especially divisions 17–25, Robinson pp. 651–654, Skeat, pp. 407–409.

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P.S. *Encyclopaedia Britannica* (14th ed.), Vol. 18, p. 735, says of Ptolemy's *Syntaxis*, that Bk. VII "ends with the catalog of the stars of the northern hemisphere in which are entered the latitudes, longitudes and magnitudes." Ptolemy was of the second century A.D.

QUESTION AND ANSWER

Science! true daughter of Old Time thou art!
  Who alterest all things with thy peering eyes.
Why preyest thou thus upon the poet's heart,
  Vulture, whose wings are dull realities?
How should he love thee? Or how deem thee wise,
  Who wouldst not leave him in his wandering
To seek for treasure in the jeweled skies,
  Albeit he soared with undaunted wing?
Hast thou not dragged Diana from her car?
  And driven the Hamadryad from the wood
To seek a shelter in some happier star?
  Hast thou not torn the Naiad from her flood,
The Elfin from the green grass, and from me
  The summer dream beneath the tamarind tree?

EDGAR ALLAN POE

Indeed, the old gods play their part and die;
  But from their graves the poet sadly turns
To see a new Diana mount the sky,
  In whom the fire of warm ambition burns
To ride not only with the moon long dead
  But far beyond the kingdom of the sun
Where Jove and all his cohorts dared not tread.
  She weighs vast worlds and counts the distance run;
Then turns to smallest things, such elfin mites
  As never have been seen but from her car.
Does Fancy shun the road this Goddess lights?
  Nay, Truth and Fancy both her handmaids are;
Their task to live, to love, to dream—with *you*
  O Poet—and then, to make the dreams come true!

RALPH HAZLETT UPSON
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